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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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Volume 14 JUNE, 1950 Number 2

EDITORIAL

PARAPSYCHOLOGY AND BIOLOGY

PARAPSYCHOLOGY, the science that deals with the place of personality in nature, is a branch of biology, broadly conceived. As far as we know, personality is a function only of *living* beings, and the science of living beings is biology. In defining parapsychology in terms of living things, however, we do not intend to ignore the question of whether personality has any existence beyond the end of life, whether there is an extrabiological function in man that is capable of operating independently of the living body. If such an independently surviving spirit in man were to be scientifically established, this would mean that a new order of reality would have been introduced beyond the present boundaries of the field.

Our present concern, however, need not involve this question of the possible extrabiological nature of man's personality. We are more interested here in a consideration of the findings of parapsychology and biology in terms of the significance each field has for the other. Even though the suggested interrelations of the two fields must of necessity be viewed as rather speculative at present, a consideration of them is worth while. Such reflections may have some guidance value to the research worker in opening up the new vistas for research that lie on our common borders.

What significance have the findings in biology for parapsychology? A certain added perspective is given to us in parapsychology when we recognize that, along with other life phenomena, the

processes which we study must have had an evolutionary origin. A direct inference from this is that they might reasonably be expected to occur in other species than man and may well have some sort of organic accompaniment, or at least a genetic foundation. Impetus is given to still more generalized reflection by Dr. Edmund P. Sinnott's recent McNair Lecture on the topic: "Life, Mind and Spirit." In this lecture Dr. Sinnott said that the regulating forces of the living organism and the mental system of the person give evidence of belonging to the same continuum. It should make a great difference in research approach if the parapsychological inquirer realizes that the forces with which he is working may conceivably be operating throughout all the life reactions of the universe.

Merely to introduce the problem of the evolutionary origin of psi ability at once raises a number of important research questions that should be answered: To what extent may psi phenomena be found in lower species? Do psi abilities in man appear to be like new evolutionary acquisitions, or like vestigial powers on the evolutionary wane? If ESP ability is found in other species than man, what organic (especially nervous) structures seem to accompany it? If psi powers have any relation to organic evolution, as presumably they must, they have to be inherited. What is this hereditary basis?

These questions focus attention upon the problem of psi abilities in different species. In the more impressive anecdotal material of parapsychology are some cases of apparent ESP experiences in animals, most of them concerning dogs. The most arresting of such accounts tell of the return of lost pets to their former homes after having been moved away—in some cases to a great distance. There are even in several instances reports that tell of the dog's successful search for its human companions after they had moved far away into new country and left the dog behind.

Such stories need not, for our purposes, be completely authenticated cases. The proper use of reports of such spontaneous performances is rather to guide the research explorer to a productive idea or insight, leaving the verification to later experimentation. They not only provide us with something to explain but they also furnish suggestions as to how to go about the search for an explanation.

Let us consider an example in which the animal reaches his master after weeks of travel over hundreds of miles, through completely new territory, with no initial orientation or guidance. What does it suggest? First, of course, that the dog has ESP ability. There is no known sense organ that is applicable to direction-finding of this order, and neither is there any known physical basis for a sensorial connection between dog and man over a thousand or even over only a hundred miles. This type of case so clearly suggests the possible operation of ESP capacity in dogs that it would be blindness indeed if parapsychologists were to fail to follow this lead. The only experimental work with dogs on record at present is the pioneer research carried out by Bechterev.

A second suggestion from the dog-homing cases lies in the extraordinary reliability of the apparent ESP function. We know from experimentation with human subjects that distance is no limitation to ESP; hence the long distances involved in some of the dog performances is not a new feature. But the fact that the animal can, hour by hour, day by day, and for months on end, make the predominantly correct turnings around mountains and lakes and over rivers and hills, and find his former owner in the heart of a great city suggests in nature's own dramatic way that here is psi ability uninhibited. If we are getting and interpreting these reports correctly, they indicate that man's ESP ability is a mere vestige of the powers these dogs display. One of the most remarkable reports concerns a homing trek by a dog covering a six-month period, with probably two and a half months of direct homeward travel. If a single human being could gain such completely reliable mastery over ESP powers in himself, what he could accomplish would tax the imagination. Compared with the dog's reported achievement, the weak, elusive, unreliable ESP capacity in man is only a fragment of what it might be.

A third suggestion about ESP is given by these reports of homing dogs. There are perhaps few examples of stronger motivation, if such values can be compared, than the sentiment of love which the exceptionally affectionate dog has for his human companions. Some homing dogs are said to arrive at their destinations in a state of exhaustion, starved, and with bleeding feet—mute evidence of

an extraordinarily powerful purpose. Perhaps the dog's devotion and his enforced separation unlock an order of psi powers that would lie dormant under ordinary circumstances.

This comparison of dog and man in spontaneous situations suggests that ESP capacity may be of early evolutionary origin. In man the evolution of the larger cerebral cortex with its large association areas has presumably developed reason at the expense of the nonrational means of orientation. That may mean that ESP is losing out, being overlaid with an elaborate intellectual system that seldom allows it to break through into free operation. If this suggestion is a correct one, it is extremely important.

The view that ESP may be a function of earlier evolutionary origin is supported by the experimental results. Rational thought and intellectual habit seem to interfere with the operation of psi processes—hence the position effects which largely limit success to the first trials in a run (the presumption being that each trial calls up associations that obscure the operation of ESP), and hence the advantage of diverting the subject from an intellectual approach and fostering a lighter, game-playing attitude in the experiments. But most important of all, the hypothesis of a more remote evolutionary origin for ESP is consistent with the observation that ESP is unconscious in its operation, that is, it functions below the reach of introspection. This unconsciousness in ESP is, of course, one of its most familiar features and may even be an essential characteristic.

We must, of course, distinguish here between unconsciousness and lack of conviction. Whatever guided the homing dog in the right direction seems often to have taken him scores of miles out of his direct pathway to find a mountain pass. But this need not imply any *consciousness of the perceptual experience* that oriented him toward home. That such compelling convictions can sometimes be experienced by human beings in spontaneous ESP is strongly suggested by large numbers of case reports; but the experience, if conscious at all, seems to be translated into a hallucination or dream. There appears to be no awareness of anything like the sensation on which sense perception is based.

We cannot easily raise the question of the evolutionary origin of an *un*conscious *extra*sensory mode of perception without suggest-

ing by implication that the evolution of *consciousness* may have resulted from evolution of the *sensory* functions and their derivatives. Consciousness is still too much of a mystery to warrant more than a speculative fling at such a suggestion, but it is worth considering that the first insight into the evolutionary origin of consciousness could well come incidentally out of a penetrative study of the evolution of the unconscious orientative function we call ESP.

If we find nothing above the canine level of brain evolution to equal the dog's demonstration of ESP, let us look below it and take up the question of possible ESP function in birds. As zoologists generally recognize, the explanation of the direction-finding and direction-maintaining capacities of some migratory species of birds still lacks a good hypothesis. In an epilogue to Rowan's *The Riddle of Migration*, Dr. A. Landsborough Thomson was quoted as saying: "To study the migration of birds is to . . . probe the inmost mysteries and to ask the very meaning of Life itself."[1]

There have been recent attempts at a hypothesis of migration based on the bird's use of two supposed sensory responses: one a response to the earth's magnetic field, and the other a response to the coriolis force, which corresponds to the lines of latitude. Thus far the hypothesis is still awaiting an acceptable supporting experiment. Theoretically it seems inadequate to account for the case reports of bird flights over territory unknown to the birds. Cases such as that of the return of a homing pigeon from Indo-China, whither it had been taken by ship, to its cote in Arras, France, would appear to require some explanation other than the hypothetical sense mentioned. There is, therefore, a much more plausible case for ESP as the orienting capacity in birds and fishes than there is for any other hypothesis, and one that is good enough to warrant a series of exploratory researches to put it to the test. In fact, no other explanation is at present logically adequate. A special merit of the hypothesis that ESP is a factor in migration is that it is capable of being tested, although not in the unpretentious laboratory where parapsychological research is usually done.

The difference in ESP ability in man and the dog as suggested

[1] *The Riddle of Migration* by William Rowan, Baltimore: Williams and Wilkins Co., 1931.

by the homing cases now recorded would seem to indicate that the simpler nervous system of the dog may facilitate the expression of ESP, or rather, interfere with it less. Birds and fish, then, by reason of their relative lack of inhibitory brain structures, may be still better equipped for the use of ESP ability than dogs. Beyond birds and fish, too, are the still simpler organisms whose homing and other behavior still includes many mysteries for which psi capacity is the only explanation we can suggest at the present time.

* * * * *

It is obvious that biological findings are of much potential significance to the field of parapsychology, but the advantages of cooperation in the two fields does not end here. Biology in turn should profit from parapsychological research. It would be as important to one field as to the other if ESP were found to be an active factor in migration, if its evolutionary origin and development could be traced, or if specific organic and genetic foundations could be found to accompany the capacity.

A scientific biology will of course have to take into account any factor that makes for successful adaptation and survival in the individual and for the modification of the species in general. Professor A. C. Hardy boldly stated in his last year's address to the British Association for the Advancement of Science that telepathy would have to be taken seriously in the consideration of organic evolution. We might add that all psi capacities that serve in any way the needs of the organism become of necessity factors both in the life of the individual and in the evolution of the species. It could hardly be otherwise.

In order to know whether to consider ESP capacity as an element of influence in evolution we need know only that a species possesses it to some degree. The uses might vary and range widely, but any additional aid to the perceptual powers of the animal, such as ESP could furnish, should give it clear advantage over its competitors in the battle for survival.

The psychokinetic or PK type of psi effect should have significance for biology that is comparable to that of ESP. One can only generalize hypothetically as yet, since the researches on PK on which we are ready to draw conclusions are still limited to tests with mov-

ing cubes and discs. It is not unreasonable, however, to extend the PK principle hypothetically to an effect on other moving objects in nature, including the moving matter in life processes. Since we are trying to arrive at a useful perspective and not to draw conclusions, there is every advantage and no risk in following up the logical conclusions of extending the PK hypothesis into the realm of the living organism.

The PK principle gives us a possible explanation for many mysteriously baffling phenomena in nature. It has been suggested that PK may be the method by which the thought-brain process is exercised in ordinary experience and behavior. We still have no explanation for this process. Also, PK may function in a wide range of organic effects of psychological origin and could conceivably be the process needed by the Lamarckian hypothesis of organic adaptation to striving. It might especially help to account for the change in the germ cells required by the theory of the inheritance of acquired characters. Just hypothesis, yes; but without a starting hypothesis, research cannot advance.

Now that medicine has more or less accepted the empirical fact that certain types of disorders are psychosomatic, it is worth considering how far PK may be involved in the psychogenetic causation of organic effects. Among the case records a fair number of outstanding instances are reported in which something that looks like a PK effect occurs. For example, in the removal of warts by means of folk magic or charms (it is now accepted practice in psychosomatic medicine to use suggestion) there are instances in which the patients are very young children to whom the suggestion theory is less applicable, and even some domestic animals which, it is reasonably certain, are not merely responding to suggestion in the ordinary sense of the term.

Back of these challenging problems of medicine is a whole array of unexplained cases of biological effects that seem to involve a psychokinetic principle. There are, for instance, the reports of the experimental production of blisters by hypnotic suggestion, with the subject under continuous observation. There are instances on record of organic effects of desire in which females of both canine and human species have developed lactation under the stress of a need

to care for suckling young. There are enough cases of suspended animation reported by professional men in such fields as anthropology and psychology to justify serious inquiry. Dermographia, or skin-writing, especially telepathic dermographia, which has some supporting evidence, seems to be best explained by PK if the reports are accurate. We do not know, of course, what foundation there is for these claims; no conclusive case can be made until this whole area is scientifically investigated, but quite obviously the PK hypothesis offers a logically possible starting-point for an explanation in these situations.

Perhaps the most valuable contribution of parapsychology to biological science in general is the one that is most unique in parapsychology and incidentally the one that has encountered the most resistance with regard to scientific recognition. That is the fact that psi phenomena do not show any relation to the physical criteria of space and time and seemingly must be taken to be extraphysical in nature. Let it be remembered that the experimental evidence underlying this conclusion, even though it occurs in a psi test, represents a living organism's response to environment. Translated into biological terms, it means that the organism has certain functions of nonphysical character and that biology therefore has to do with forces that do not follow the known principles of mechanics.

This finding has finally blasted the lock of materialism off the gate of the biological sciences. It has opened a new range of hypotheses that may help to clear up some of the biological puzzles that will not yield to mechanistic explanation. Fear of the metaphysical complications of a dualistic vitalism has driven biologists to hug the barren wall of strictly physicalistic theory, even at the cost of letting their greater problems remain unsolved. Now, already established by experiment with a degree of rigor and control that ranks with the best experiments in biology, psi capacity stands as a biological fact. What its further significance for general biology may be remains of course to be explored, but what it has to show is revolutionary.

Psi research has raised the possibility that the whole organic system may be unified by more than electrochemical processes, protoplasmic strands, and synapses. We still do not know the nature of the organizing, directing, self-maintaining forces in the living organ-

ism. This may mean that we have been looking in the wrong direction. Every thoughtful biologist will accept the suggestion that there still must be unknown forces in the living organism. Once he is assured that such forces have to be in some way interactive and unifiable with known physical processes (oxidation, osmosis, etc.) he may even be willing to concede that these forces do not have to be just like the already familiar ones. There is a great exploratory advantage in having diversified hypotheses. If we act on the hypothesis that all the energies of the cell follow the same principles, the scope of possibilities for investigation is greatly circumscribed. Consideration of the psi processes, even in the limited extent to which we now know them, opens up a great range of hypothetical research possibilities in the field of biology.

The trouble, of course, is that biologists will find it hard to take these suggestions seriously. That is the reason why parapsychology itself will have to do the spade work, the first explorations. A first step is an excursion into the application of psi tests of various types to living organisms, animal and plant. These tests may give results that will make the next step easier for the biologist himself to take. The greatest caution, of course, is needed in this bold branching-out, but it is needed in the drawing of conclusions rather than in the consideration of problems and possibilities. Anything can be tried with safety if the test methods are adequate. The biologist, then, need not fear trying out something that does not square with chemistry or physics. A little more venturesomeness may help to clear up at least some of the fundamental problems of life.

It is a question how far a cry it is from psi phenomena to the basic life determinants. Something in the fundamental biology of the living creature does, we know, appear to defy physical law in a number of ways. Life is growth, not a leveling process; it involves the concentration of energy in defiance of entropy; it is the building-up of a system, and the organization of forces and molecules, not decline and dissolution. This basic life force is unconscious, but directive; it is one with the space-time-mass system of matter, yet more than nonliving matter. Parapsychology, in accordance with Dr. Sinnott's generalization, may have broken through the invisible walls of conventional thinking that have hampered us and found a

connecting passageway that leads right through from hypothetical "spirit" (or nonphysical mental reality) to some basic primordial life energy. Or perhaps we might better say it may have found an opening that could lead to such a passageway.

J. B. R.

A FURTHER STUDY OF ESP DISPLACEMENT IN RELATION TO HITS AND MISSES

By J. G. Pratt and Esther Bond Foster

ABSTRACT: It was shown in a previous report that D.W., one of the high-scoring subjects in the Martin and Stribic experiments, sometimes "displaced" or spread her ESP to target cards coming before or after the one at which she was aiming.

In the present report the data of another high-scoring subject, C.J., from the same experiments, were analyzed for displacement effects, and trends similar to those shown by D.W. were found. The most significant displacement result in C.J.'s data was in the groups of successive trials where no direct hits occurred. In these missing areas, the calls missed the target cards one step ahead (forward displacement) to a significant degree, while there was a slight tendency for calls to make more than the expected number of hits on the target cards one step behind (backward displacement). The negative deviation on forward displacement could be interpreted in terms of ESP, but the positive deviation on backward displacement was explainable in other terms.

Analyses were made to throw further light upon the negative forward displacement in the missing areas. The displacement effect was found to be primarily centered in the pairs of misses coming immediately after direct hits, though the displacement result in the remaining pairs of misses was also statistically significant. The focusing of the displacement effect in the pairs of misses after hits suggests that the subject was reacting to his ESP success.

The authors stress the importance of still further investigation.—Ed.

We reported in the March Journal some new findings regarding ways in which ESP occurs in card tests (3). These results were concerned with ESP displacement in the data of D.W., one of the high-scoring subjects of the Martin and Stribic experiments at the University of Colorado (1). The calls that missed the direct targets, those the subject was trying to hit, were found to be related to targets coming later in the card series. Whether the calls that were wrong for their direct cards made more or fewer than the expected chance number of hits on the next targets depended upon whether those targets were hit or missed by their own calls, the ones the subject intended for them.

In the areas of the data in which two or more misses on the direct targets occurred in succession, the number of hits found when calls were compared with the immediately following cards (forward

displacement) was significantly lower than the number of hits made by calls on the immediately preceding cards (backward displacement). On the other hand, the opposite direction of difference between forward and backward displacement was found for wrong calls coming next to hits. Wrong calls *preceding* direct hits gave a forward displacement score that was significantly higher than the backward displacement score from wrong calls *following* direct hits.

This line of investigation has been continued with two immediate objectives. First, we have been interested in surveying all of the ESP card data available to see to what extent the finding is a general characteristic of ESP performance. Secondly, we want to throw as much light as possible upon the nature of these effects by means of analyses made upon the data in which they are found. Ultimately, of course, the value of the findings must be judged by how fruitful they are in future experiments.

To date, a number of subjects' ESP records have been examined for displacement in relation to hits and misses. There is now ample evidence, most of it yet to be reported, that other high-scoring ESP subjects exhibit significant displacement effects similar to those reported for D.W. The most interesting findings have come from the work of C.J., the major Colorado subject, and the investigation of his extensive ESP data is not yet completed. As these findings are reported, it will become increasingly clear that these displacement effects are statistically significant. How they are to be interpreted is the important question. As each new report is made, therefore, the chief aim will be to uncover new suggestions regarding the nature of these effects and to test, so far as possible, hypotheses that have already been formulated on the basis of previous studies.

The present report will deal with a series of 300 runs by C.J. These data have not previously been discussed in this connection, although some other work by this subject was included in the earlier paper (2) on the related topic of change of call (the tendency to repeat a call symbol with different frequencies after hits and misses). The 300 runs to be considered here are those referred to in the detailed report by Martin and Stribic as "Minor Variations" in which the screened DT ("down through") clairvoyance test procedure was used. This series came just after the 1000-run Main DT Series by

C.J. (900 available runs of which were studied in the change-of-call paper) and just before his 1000-run UT ("up through") series. For convenience, we shall refer to the 300 runs under investigation here as the B Series, the "B" indicating "between" the other two longer series.

All of the 300 runs of the B Series were tests for clairvoyance done with the ordinary ESP deck of 25 cards, five of each of the five symbols. The DT procedure was used throughout, and the target cards were always screened from the subject's sight from the time they were shuffled until he had completed his calls. The total direct score, or the number of hits made by calls upon the targets for which they were intended, was 2,191, a deviation of +691 from the expected average. This is an average direct score of 7.30 hits per run, and the CR for the series is 19.95.

The B Series shows a highly significant difference between forward and backward displacement within those groups of trials in which no direct hits occurred. This particular displacement contrast, with which the present report is chiefly concerned, is stronger than that found in the D.W. series. It should, therefore, be possible to gain some further insight into what happened and why. But there is an additional reason why a thorough exploration of this effect is especially appropriate for the B Series. The two longer series by C.J. done before and after this one will afford an opportunity to check any suggestive findings regarding the nature of the ESP displacement that may emerge from an exploratory study of the B Series.

Results

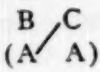
Total Displacement Results

As in the D.W. study, the 300 runs of the B Series were checked for displacement in relation to hits and misses on the direct targets. The calls to be checked for displacement were separated into two groups: those that hit their direct targets and those that did not. Likewise, the targets were separated into two groups: those that were hit by the calls intended for them and those that were not. Thus the displacement check involved four possible groupings of calls and targets:[1]

[1] As in the previous paper, call and card situations will be illustrated by the

(I) A wrong call and a missed target:

(II) A wrong call and a hit target: and

(III) A correct call and a missed target: and 

(IV) A correct call and a hit target:

The first two groups are of the most interest as far as displacement in a high-scoring subject is concerned, as they deal with calls that missed their own targets. We would like to know whether such calls score differently in displacement when they are checked against neighboring targets that were missed and those that were hit by their own calls.

The calls of the B Series were checked for ESP displacement in terms of the four types of relationship given above, and this was done for the targets that were one and two places away from the calls, on both sides of the direct targets. The most significant contrast was the difference between the (+1) forward displacement and (−1) backward displacement scores within the areas of the data in which no direct hits were made. The missing areas provided the same number of opportunities for forward and backward displacement since two misses in succession give one "trial" each of (+1) and (−1) displacement, three misses in succession give two "trials" for displacement in each direction, and so on.

In the entire B Series, there were 3,686 comparisons of wrong calls and adjacent misses for forward displacement and the same number for backward displacement (that is, 3,686 pairs of misses as in the first example shown above). In these pairs of misses the first call hit the second card 608 times. On the other hand, the second call hit the first card 879 times. In checking the wrong calls against adjacent missed cards for displacement, therefore, the (+1) score was 271 *lower* than the (−1) score.

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use of the letters, A, B, C, D, and E. When both call and target symbols are shown for two or more successive trials, the calls will be shown on the left and the cards on the right. Hits on the direct targets will be enclosed within parentheses. Diagonal lines will be used to show the displacement relations under consideration, and the diagonal line will have an arrow point when a displacement hit is indicated.

The direction of the difference between forward and backward displacement was found to be reversed when wrong calls were checked against adjacent hits (as in Type II above). The (+1) score in 1,402 instances in which wrong calls came just before hits was 209, while the (−1) score in 1,429 cases in which the wrong calls came just after hits was 183. In this comparison, in spite of the fact that there were a few more opportunities to make (−1) hits, the forward displacement score is 26 *higher* than the backward displacement score. These and other displacement results found in the B Series are summarized in Table 1.

Table 1

SUMMARY OF DISPLACEMENT RESULTS IN THE B SERIES—SUBJECT C.J.

	Type I Wrong calls checked against missed targets A B / C D		Type II Wrong calls checked against hit targets (+1) = A B / (C C) (−1) = (C C) / A B		Type III Correct calls checked against missed targets (+1) = (A A) / B C (−1) = B C / (A A)		Type IV Correct calls checked against hit targets (A A) / (B B)		Total displacement score	
Displacement	"Trials"	Hits	"Trials"	Hits	"Trials"	Hits	"Trials"	Hits	"Trials"	Hit
(+1)	3686	608	1402	209	1429	213	683	78	7200	1108
(−1)	3686	879	1429	183	1402	267	683	78	7200	1407
Diff. (+1)−(−1)		−271		+26		−54		0		−299
(+2)	3524	838	1337	61	1417	276	622	28	6900	1203
(−2)	3524	908	1417	73	1337	307	622	28	6900	1316
Diff. (+2)−(−2)		−70		−12		−31		0		−113

Examination of the results on displacement in relation to hits and misses shows that the difference of 271 between (+1) and (−1) for wrong calls checked against adjacent misses is by far the most striking contrast found. This is the difference we now wish to examine more closely.[2] Unless otherwise indicated, therefore, when

[2] By singling out this result, we do not wish to imply that there is nothing of interest in the rest of the table. The following observations merit further consideration as opportunities arise: First, the difference between (+1) and (−1) under Type III is very interesting, especially as it follows the pattern of (+1) and (−1) in Type I. A similar result was noted in the D.W. report (3). Second, the observation that the direction of difference between (+1) and (−1) in

we speak of displacement in the rest of this report we shall be referring to the (+1) and (−1) results based on the pairs of misses.

The statistical significance of the displacement results may be evaluated in terms of the difference between the (+1) and (−1) scores. This may be done by observing how the total difference is distributed through the entire series, run by run. The chance hypothesis is that the most likely difference between (+1) and (−1) in a run is zero. If chance factors alone are a sufficient explanation of the results, the distribution of 300 "run" differences for the series would be a random sample drawn from a normal distribution with a mean of zero. Therefore a *t*-test may be applied to see whether the observed mean is significantly different from zero. The observed mean of this distribution of differences or the average difference per "run" is, of course, 271/300 or an average "run" score on forward displacement that is .903 lower than that on backward displacement. The 300 run differences were squared individually and added; this sum was 1,571. The *t*-value found for the average difference between (+1) and (−1), where a zero difference was expected, was 7.5, with 299 degrees of freedom. A difference as large as the one observed would be expected as a chance occurrence less than one time in 100,000,000,000 such series. Chance may therefore be dismissed as the explanation.

It is important to know to what extent the two directions of displacement, (+1) and (−1), contributed to this significant difference. Each displacement score was evaluated separately by the method of approximation described in the appendix of the preceding report. The expected (+1) score was found to be 817 hits; therefore the observed score of 608 (+1) hits represents a deviation of − 209, and the CR is 8.4 ($P = < 10^{-14}$). The expected (−1) score, on the other hand, was found to be 820 hits. The observed (−1) score of 879 thus gives a deviation of +59, and the CR is 2.4 (P = .016). The difference between (+1) and (−1) is therefore

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Type II is a reversal of that in Type I may be of great psychological importance in spite of the fact that the Type II difference is not independently significant in the B Series, as it was in the D.W. data. Third, the (+2) and (−2) difference in Type I is large enough to attract attention and suggests that it may be important to study displacement effects in relation to hits and misses over greater spans of separation between calls and cards in the longer C.J. series awaiting analysis.

chiefly due to the fact that the forward displacement score is below chance expectation.[3]

The marginally significant positive deviation on backward displacement has a logical explanation as an effect of two factors taken in combination. One is the avoidance score on forward displacement; the other is the fact that the subject almost never called the same symbol twice with only one call symbol in between, as in A B A. His sequence of calls shows approximately three per cent of these patterns, where 20 per cent would be expected in a random series. When three misses occurred in succession and the first call avoided forward displacement to the middle card, the fact that the subject rarely called the same symbol on the second trial ahead meant that the probability of getting a backward displacement hit on the middle card was about 1/4 instead of 1/5. As a result, the backward displacement score would be raised, and when the deviation is calculated on the 1/5 probability basis, the expected positive deviation on backward displacement would be about one-fourth as much as the observed negative deviation on forward displacement. It is

[3] The slight difference between the computed expected scores for (+1) and (−1) is due to the fact that the method takes the score made on one displacement into account in calculating the expected score on the other, as there is a slight interdependence between the two. The method is an approximation in that it assumes that the same number of direct hits was made on each of the five target symbols. More exact methods that have been applied for estimating the (+1) and (−1) expectations indicate that the expected values given above may be from 20 to 30 hits too high. All of the methods agree in leaving the negative deviation on forward displacement as the outstanding effect.

The computational formulas of the method applied above are as follows:

$$\text{Expectation}_{(+1)} = (5/19)(x - b) + (1/4)(b);$$

$$\text{Variance}_{(+1)} = (5/19)(14/19)(x - b) + (1/4)(3/4)(b);$$

$$\text{CR}_{(+1)} = \frac{\text{Obs. Score} - \text{Expt.}}{\sqrt{\text{Variance}}}$$

And

$$\text{Expectation}_{(-1)} = (5/19)(x - a) + (1/4)(a);$$

$$\text{Variance}_{(-1)} = (5/19)(14/19)(x - a) + (1/4)(3/4)(a);$$

$$\text{CR}_{(-1)} = \frac{\text{Obs. Score} - \text{Expt.}}{\sqrt{\text{Variance}}}$$

In these formulas x is the number of pairs of misses with the two call symbols different from each other (that is, they do not form a double), of which there were 3,146 cases in the B Series; a is the number of forward displacement hits, or the (+1) score; and b is the number of backward displacement hits, or the (−1) score. Readers interested in the statistical basis of the evaluation are referred to the appendix of the preceding paper.

evident from the expectations given above that the observed results agree with this logical prediction.

It is clear, therefore, that what we are chiefly concerned with is the question of the meaning of the negative score on forward displacement. The fact that the (+1) and (−1) scores are dependent upon each other to this extent does not, however, invalidate the basic evaluation of the displacement effect in the miss areas of the data by means of the *t*-test, as on the chance hypothesis the two scores are expected to be equal. In some of the further analyses made for the purpose of studying the negative deviation on forward displacement, the results will still be analyzed and evaluated in terms of displacement differences. This practice will be a matter of convenience only and need not obscure the fact that the (+1) effect is the one of primary interest.[4]

Comparison of Displacement and Direct Scores

One of the first questions that arises is how these displacement effects are related to the degree of success in hitting the direct targets. Is the displacement a form in which a subject's ESP ability shows up when he is failing to score above chance on the targets at which he is aiming? Or is it that the success in hitting the direct targets and the displacement effects are influenced by the same factors but in opposite directions: the higher the direct score, the further the forward displacement below chance expectation? Still a third possibility is that there is only a random relation between these two ways in which ESP ability is demonstrated.

To find which of these possibilities is correct for the B Series, we made a study of the (+1) and (−1) scores in relation to run

[4] The basic evaluation of the displacement difference has been made by means of Student's *t* as this method avoids the assumption that the (+1) and (−1) scores are statistically independent variables. The chi-square analysis is more convenient to apply, but this method does involve the assumption of independence. However, we have made a number of comparisons between the degree of significance obtained by the *t*-test and by a chi-square analysis of the same data, and the results have always been approximately equal. In the present series, for example, the *t*-value obtained was 7.5, while the chi-square (one degree of freedom) was equivalent to a CR of 7.9. Since the error introduced by the use of the chi-square appears to be quite small, we have used this more convenient method for the approximate evaluation of the displacement effect in different groupings of the data. Later in the paper when we compare special groups of pairs of misses for displacement effects, we often do not have precisely equal expectations for (+1) and (−1), but we have been careful in each instance to see that any error introduced would reduce the significance of the analysis.

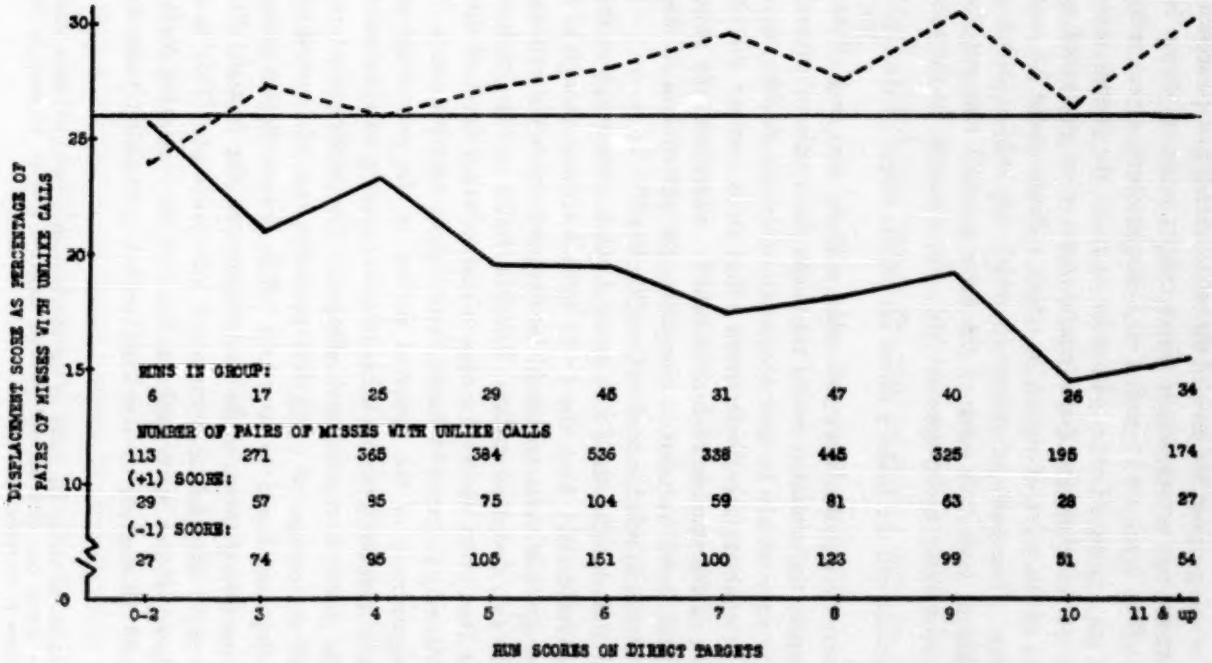
DISPLACEMENT SCORE AS PERCENTAGE OF PAIRS OF MISSES WITH UNLIKE CALLS
30
25
20
15
10
0
RUNS IN GROUP:
6 17 25 29 45 31 47 40 26 34
NUMBER OF PAIRS OF MISSES WITH UNLIKE CALLS
113 271 365 384 536 336 445 325 195 174
(+1) SCORE:
29 57 95 75 104 59 81 63 28 27
(-1) SCORE:
27 74 95 105 151 100 123 99 51 54
0-2 3 4 5 6 7 8 9 10 11 & up
RUN SCORES ON DIRECT TARGETS

FIG. 1. Forward displacement (solid line) and backward displacement (broken line) in relation to run scores on the direct targets. Each curve shows the percentage of displacement score in the pairs of misses with unlike calls. The data upon which the graph is based are shown.

scores. For this purpose, we divided the runs into ten groups according to what the run scores on the direct targets were, as shown in Figure 1. The total (+1) and (−1) displacement scores were found for each group of runs. In order to make the groups comparable to one another, the displacement scores were expressed as percentages of the pairs of misses in which a displacement hit was a possibility. The pairs of misses in which the subject called a double (that is, both calls were of the same symbol) rule out the possibility of making a displacement hit. For example, in the pair

A B
A C

neither call can make a direct hit on the target of the other trial without having the other call make a direct hit; and if this should happen, the situation would no longer be a pair of misses. As subjects vary widely in their frequency of calling doubles, it is desirable to evaluate the displacement effect in a manner that is independent of the number of doubles called. Therefore only pairs of unlike calls were included in computing the percentages of displacement scores in relation to direct-scoring level.[5]

Figure 1 shows that the (+1) score drops in percentage as the direct score increases. Also, the (−1) score increases, though at a much more gradual rate, as would be expected from the relation between the two described earlier. The line drawn on the graph at the 26 per cent point shows the approximate average level of displacement scoring expected if chance factors alone were present.

The significance of the apparent decline in the percentage of (+1) hits in relation to the ten levels of direct scoring was evaluated in terms of a simple regression coefficient. The coefficient of regression of percentage of (+1) hits upon the rate of increase of direct scoring was found to be −1.06. This means that in going from one score level group to the next higher one, the forward displacement score dropped an average of 1.06 per cent. This is a statistically significant relationship, as the t of the regression coefficient is 5.48 (8 degrees of freedom) with a probability value of .0003.

[5] This practice of basing the study of the displacement scores upon those pairs of misses in which two different call symbols appeared will be followed in the further analyses to be reported.

Displacement Effects in Proximity to Hits

The previous section shows that there is an inverse relation between direct scoring success and the percentage of forward displacement hits in the pairs of misses. This finding leads to further questions about the nature of the relationship. Is the connection between direct scoring success and degree of avoidance on (+1) a general one for the high-scoring runs, or is there a more specific connection? That is, is the displacement uniformly distributed over the pairs of misses within a given run, or will the strength of the effect depend upon where the pairs of misses come in relation to hits?

In the course of seeking an answer to these questions, we checked the displacement in the first pair of misses following a hit. A much stronger contrast between the (+1) and (−1) scores was found there than in the remaining pairs of misses. After hits, there were 809 pairs of misses with unlike calls, the cases in which displacement was possible. These gave a (+1) score of 114 (14.1%), and a (−1) score of 247 (30.1%). By contrast, the 2,338 other pairs of misses with unlike calls gave a (+1) score of 494 (21.1%), and a (−1) score of 632 (27.0%). There is a 16% difference in displacement scores in the pairs after hits, and a difference of 5.9% in the other pairs. The pairs after hits make up only 25.7 per cent of the total number of such pairs, but they account for 49.1 per cent of the total difference between the (+1) and (−1) displacement scores.

As a general measure of the difference in displacement effect after hits and elsewhere, a chi-square evaluation was made. A fourfold table analysis was applied to those pairs of misses that showed only one displacement hit, either (+1) or (−1). The pairs of misses that showed neither direction of displacement as well as those that had hits in both directions were excluded, as they could not influence the *difference* in any way. Since they would simply have increased the number of cases, the inclusion of these pairs would have had the effect of making the chi-square analysis less sensitive than necessary. The groupings of the data used and the results of the evaluation are as follows:

Displacement Hits	Pairs of Misses: After Hits	Pairs of Misses: All Others	
(+1) Only	77 (110.2)	372 (338.8)	449
(−1) Only	210 (176.8)	510 (543.2)	720
	287	882	1169

$\chi^2 = 21.56$ (1 d.f.) $P = .000003$

The chi-square indicates that the displacement difference is especially characteristic of the pairs of misses coming just after hits. As this analysis was exploratory, it is necessary to consider the degree to which the result might be due to favorable selection among many possible comparisons. The analysis of the first pair of misses after hits was one of the first ones made in the effort to throw further light upon the displacement difference. The result obtained was not, therefore, the outcome of trying a large number of combinations until a lucky one was found. Also, the probability would still be significant if it were corrected for selection from among a thousand analyses. Actually not more than ten were even considered. In spite of the exploratory character of the result, there are adequate grounds for accepting as demonstrated the fact that the pair of misses after a hit in the B Series is a special focal point for the displacement difference. This makes it all the more important to examine even more closely the displacement in relation to hits both in this series and in others.

A study of the displacement effect in proximity to hits was made to see how the pairs immediately following hits compared with pairs immediately preceding hits. In this tabulation every case in which a pair of misses came between two hits was excluded from consideration as there was no basis for classifying these pairs as either "after hits" or "before hits." Also, at the time this analysis was made, we already had some reason (as explained in the next section) for thinking that there was some factor associated with incorrect double calls that might have an influence upon the displacement effect. We therefore also disregarded pairs of misses that were unclassifiable because it could not be said whether they were related to an adjacent

hit or a wrong double in the calls. In addition to the single pairs of misses between hits, the following cases were therefore excluded: a pair of misses that came between a wrong double and a hit, whether the pair overlapped the double or was adjacent to it; and a pair before or after a hit when the correct call itself was a member of a double. (The significance of wrong doubles will be considered more fully in the next section. In a particular sequence involving a hit or a wrong double, the tabulation in each of these analyses was done for any pair or pairs that qualified, even though there was an incomplete sequence to be recorded.)

The study of the displacement effects before and after hits in terms of these strictly selected cases gave results as follows: There were 277 pairs of misses with changed calls that clearly were classifiable only as "after hits." These gave a (+1) score of 38 (13.7%) and a (−1) score of 90 (32.5%), a difference of 18.8%. On the other hand there were 260 changed pairs of misses that could be classed only as "before hits." On these, the (+1) score was 50 (19.2%) and the (−1) score, 68 (26.2%), a difference of 7%. A comparison of these percentages of displacement scores with those given above for all of the pairs after hits shows that the relative strength of the effect after hits is not changed appreciably by the more rigid basis of selection used here. The indication is clear that the peculiarity under investigation, which is especially strong just after the subject made a hit, is not especially outstanding in the pair just before a hit.

This finding is potentially of great importance, and it therefore justifies the most careful consideration of factors in the proximity of hits that might have produced or influenced it. Is there anything unusual about the distribution of calls and cards before and after hits that might affect the displacement scoring? The last tabulation above excluded the instances in which the hit was made on one member of a double call. As Table 1 shows, there are 26 more instances in which the hit came on the second of a double call (causing forward displacement in the comparison of a wrong call with a hit target) than on the first, and the elimination of these cases removes a possible inequality due to the sequence pattern of calls about hits.

In the case of the cards, there is a more serious inequality, as

the comparison of (+1) and (−1) under Type III of Table 1 shows. There are 54 fewer instances of a hit being made on the first member of a double in the cards, such as $\begin{matrix}(A & A)\\ B & A\end{matrix}$, than there are of hits on the second member of a double in the cards, like $\begin{matrix}B & A\\ (A & A)\end{matrix}$.

To equalize the situation in the proximity of hits insofar as doubles in the cards were concerned, we compared displacement effects before and after hits when, in addition to the exclusions previously described, every instance in which the hit involved a double in the cards was omitted. The remaining pairs were then evaluated by a chi-square test in terms of the proportion of forward to backward displacement before and after hits. The results were:

Rigidly Selected Pairs of Misses Adjacent to Hits That Do Not Involve Either Call or Card Doubles (See Text)

Displacement Hits	Before Hits	After Hits	
(+1) Only	34 (22.7)	12 (23.3)	46
(−1) Only	37 (48.3)	61 (49.7)	98
	71	73	144

$\chi^2 = 16.37$ (1 d.f.) $P = .00005$

The numbers of cases have been considerably reduced in this analysis. The result is still highly significant, however, and the smallness of the sample is of less concern than it would be if the result were only marginally significant. What the analysis shows is that even after prior consideration is given to every factor in the situation that seems likely to affect the displacement difference, the contrast between (+1) and (−1) displacement after direct target hits is significantly more pronounced than that before hits.

The Displacement Effect in Proximity to Wrong Double Calls

One observation made in the preceding paper on D.W. was that the difference between (+1) and (−1) displacement in the pairs of misses was attributable to the two call symbols on which the subject called a disproportionately large number of doubles. This led us

to check, after that paper had gone to press, to see whether the displacement difference was more marked in those pairs of misses coming near wrong double calls. We found that the effect was stronger in the pair just after, but not overlapping, a wrong double. For example, in a sequence of *wrong calls* like A A B C, the BC pairs showed a greater relative difference between (+1) and (−1) displacement than did the data as a whole. The contrast between the pairs after wrong doubles and the rest of the pairs was not significant, but it did stimulate us to examine the C.J. data in the same way.

A suggestively large (+1) and (−1) difference in the pair of misses following wrong doubles was found in the B Series of C.J. In this subject's data, however, the stronger effect came one step nearer the wrong double, that is, in the pair of misses of which the first call was the second of the double. In sequences of wrong calls like the one shown above—A A B C—the AB pairs were the ones that showed more than the average displacement differences. On the other hand, pairs of misses one or two steps before the double and those two steps after the double were not outstanding. The displacement results in the pairs of misses that followed and overlapped wrong doubles, regardless of what they were followed by, were considered in relation to the remaining pairs of misses *except those following hits, which have already been found to be peculiar in the large displacement difference shown.* The following fourfold table provides a test of the significance of this relationship.

Displacement Hits	Pairs of Misses: Following but Overlapping Wrong Doubles	Pairs of Misses: All Others Except After Hits	
(+1) Only	45 (55.7)	327 (316.3)	372
(−1) Only	87 (76.3)	423 (433.7)	510
	132	750	882

$\chi^2 = 4.16$ (1 d.f.) $P = .04$

The chi square is only in the suggestive range of significance, especially as it is selected as the best of four results. It does, how-

ever, justify making a further study of displacement near wrong doubles in other ESP series, particularly in the rest of the C.J. data.

The number of hits on forward displacement is significantly lower than the number from backward displacement in the general pairs of misses (that is, in those that remain when the pairs after hits and those following wrong doubles are taken out). As the figures in the fourfold table above show, there are 327 general pairs that have (+1) hits only, and 423 with (−1) hits only. On the chance hypothesis the number of pairs would be expected to be equal. The difference gives a chi square of $(423 - 327)^2 / (423 + 327) = 12.3$ (1 d.f.), which is still highly significant.

Discussion

The present study of a part of the C.J. data has confirmed the major findings of the preceding report on D.W. and there are several facts that may now be considered as established on the basis of these two investigations. Significant displacement occurred in the ESP card records of these two high-scoring subjects, but the displacement effects were not uniformly distributed throughout the subjects' calls. Instead, displacement was found to be related to the subjects' success in hitting the direct targets. In areas of the data where no direct hits occurred, both of the subjects showed a highly significant avoidance of the target card one step ahead of the direct target, the one assigned for that call. On the other hand, incorrect calls coming just before hits on the direct targets tended to anticipate the target cards one step ahead, and thus to score positively on them by forward displacement. The latter result was independently significant in the D.W. series and suggestively in the same direction in C.J.'s data.

It was found that in the B Series of C.J. the negative forward displacement score in the pairs of misses is directly related to the level of success in hitting the direct targets. That is to say, the higher the run score as based on direct hits, the lower the percentage of forward displacement found in those areas of the run where direct hits did not occur. After this relationship was discovered in the C.J. data, we re-examined the D.W. data from the same point of view. D.W. showed no relationship between the level of direct scoring and the forward displacement.

A further result coming from the present investigation is closely related to the one mentioned in the preceding paragraph. This is the fact that the negative score on forward displacement was found to be particularly prevalent in the pairs of misses immediately following hits. Discovering that the displacement effects characteristic of the miss areas are centered in the pairs of misses just after hits further localizes the ESP peculiarities we are studying. No unusual concentration of displacement in the pairs after hits was found in the D.W. data.

Another observation of interest is that made in the B Series upon the pairs of misses following upon but overlapping with a wrong double call. Like the pairs after hits, these pairs after wrong doubles were found to have an unusually low rate of scoring on forward displacement. In fact, the contrast between forward and backward displacement in these pairs was comparable in terms of percentages of (+1) and (−1) displacement scores to that found in the pairs after hits. The D.W. data also showed an interesting relation of displacement to wrong doubles, but D.W.'s most significant avoidance on forward displacement came one pair later than in C.J.'s data.

While, therefore, the D.W. and C.J. results are similar as regards the basic displacement findings, the performances of these two subjects are sharply contrasted so far as the results of these more detailed analyses are concerned. These differences between the two subjects may be facts of great psychological importance. They indicate that we are dealing with individual responses of the subjects, responses that reflect differences in personal reactions to the test situation.

The two subjects were quite similar to each other in general level of direct hitting (D.W. averaged 7.50 hits per run; C.J., 7.30) and the tests were done under virtually identical conditions, since they were both DT tests of clairvoyance based upon the ordinary ESP deck. Where we have the general displacement results in relation to hits and misses following the same pattern so closely—negative forward displacement within the groups of misses and positive forward displacement of wrong calls into hit targets—we would expect the further findings to follow the same patterns in the two subjects if the results are to be explained in terms of any statistical or pro-

cedural factors common to the two series. We find, however, that only C.J. characteristically shows the avoidance of the target one step ahead in the pairs of misses after hits. The D.W. data were rechecked for this same peculiarity, and no evidence was found for a concentration of effect in any pairs of misses in relation to direct target hits. It seems clear, therefore, that we are dealing with individual differences in adjustment and that each subject's ESP operated in a manner that was characteristic of his motivational and personality factors at the time the test was done.

Not only are there differences of this kind between these two subjects, but each subject changed in the way in which he demonstrated his ESP abilities. In the extensive records of C.J.'s performance, some coming before and some after the B Series, we have already surveyed the displacement findings sufficiently to know that they vary to an extreme degree in this subject. In the earlier work in which he scored significantly, we do not find any evidence of avoidance on forward displacement in the misses, and this type of evidence is likewise absent in later work in which he also scored significantly high on the direct targets. Apparently we are dealing here with overtones of ESP performance which vary from series to series even in the same subject, and to a certain extent vary independently of the subject's ability to score significantly on the assigned targets.

Even within a single series of 300 runs, chronological variations in displacement effects occurred in both of these subjects. In general, both subjects showed a decrease in the difference between forward and backward displacement scores as the series progressed, a decrease that was out of proportion to the variations in the level of direct scoring. In D.W., the displacement difference almost completely disappeared in the last 80 runs of the series, while the direct scoring level was still highly significant and was only slightly lower than that in the rest of the series. In the B Series, the displacement difference decreased throughout the series, and the contrast between the first and last sections was greater than in D.W. These chronological results are shown numerically in Table 2. Each series was divided into three sections made as nearly equal as possible without splitting individual sessions. A comparison of the last two columns of the table shows how the displacement difference varied in each subject within the series in relation to the level of direct hitting.

Table 2

CHRONOLOGICAL RELATION BETWEEN THE DISPLACEMENT SCORES AND THE DIRECT SCORES OF D.W. AND C.J.

Runs Grouped into Chronological Sections	No. Pairs of Misses with Unlike Calls	(+1) Hits	(−1) Hits	Diff. Between (+1) and (−1) Expressed as % of Pairs	Av. Run Score of Direct Hits
		D. W.			
1 -101.....	932	196	261	7.0	7.28
102-220.....	935	179	246	7.2	8.34
221-301.....	752	190	194	0.5	6.95
		C. J.			
1 -110.....	996	148	290	14.3	8.17
111-210.....	1134	243	333	8.0	6.55
211-300.....	1016	217	256	3.8	7.08

We are unable to offer a final interpretation of these findings at this time. We would resist strongly, however, any suggestion that we should shrink from facing the facts simply because they are puzzling. It seems likely that the more puzzling the facts, the more important the explanation when it is reached. In general, these investigations have been pursued on the principle that the only adequate treatment for puzzling facts is more facts. Until someone points out the error of this course or suggests a better one, we shall continue to follow it.

Parapsychologists generally tend to think of a significant below-chance score in ESP as indicating that a subject was actually motivated in some way to make calls that missed the targets in question. This interpretation applied to the present findings would mean that C.J. was sometimes selecting calls by ESP in order to avoid the targets one step ahead. Another hypothesis has occurred to us of the way in which the negative displacement score might have arisen, a hypothesis that does not require the concept of motivated avoidance. We mention it briefly here not as an explanation that seems preferable to the one commonly offered but as a possibility that needs to be considered.

It is possible that the negative deviation on forward displacement was not a result of motivated avoidance, but was a secondary effect of the success in hitting the direct targets. On this hypothesis, some of the displacement hits that would normally have been expected as chance coincidences were somehow "borrowed" and brought into a focal position so as to produce direct hits. By this "borrowing" process, what would normally have been a pair of misses would be removed from that category, as one of its trials would now show a direct hit. As a result, the pairs of misses that were left would contain fewer than the expected chance number of displacement hits. We do not presume to say how such a borrowing might take place, but if it did it might have the effect of leaving a deficit of hits in the one or more displacement categories involved, such as forward displacement in the B Series.

The fact that C.J. shows a significantly stronger avoidance of the card one step ahead just after his direct hits than elsewhere suggests that he may have been reacting to his ESP hits. Here we have evidence of an indirect or distorted form of ESP response coming immediately after the subject has succeeded in terms of his conscious aim. The subject was not told when he had been successful. If, therefore, on the calls following direct hits he showed this peculiar ESP displacement reaction, it looks as if he was somehow "aware" of having been successful. The reason this interpretation is suggested is that the displacement effect is significantly more marked in the pair *following* the hits.

An alternative hypothesis of the concentration of the displacement effect immediately after hits would be that occasionally there were sequences of calls when the subject's ESP was working better than at other times and that the two forms of ESP evidence, hitting the direct target and missing the forward displacement target, occurred together during such periods of grouped success. On the grouping hypothesis we would still expect to find more of the forward displacement effect after the direct hits than before them. The reason is that the first call of a pair of misses may come immediately after a hit but it could not immediately precede a hit. Therefore if grouping of ESP "successes" occurs, whatever ESP influence might be operating to bring about the negative (+1) deviation would be most likely to affect the pairs of misses after hits.

The question whether the findings indicate direct reaction to ESP success brings us back to the observation from which the present line of investigation started, as discussed in the paper on change of call in the December, 1949, JOURNAL. Evidence was presented in that report suggesting that D.W., and to a lesser degree C.J. in an earlier series, reacted to making a hit by calling a different symbol relatively more frequently on the trial following a success. The problem raised could not be solved on the basis of the data available, and the matter was left open pending further investigation. Whether a successful ESP subject has any kind of "awareness" of his ESP successes was considered at that time to be a problem of primary importance, and it is still so considered today. It may be that we have in the present C.J. series evidence that will lead to a final answer to this question.

The problem of reaction to ESP success is so important, however, that it would be unwise to claim that it has been solved before every alternative explanation has been carefully considered and reconsidered. The present report serves its purpose in keeping this problem open and to the fore among the high priority research objectives.

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SOME NOTES ON A PRELIMINARY PK EXPERIMENT WITH SIX DICE

By Ronald Rose

ABSTRACT: This PK experiment with dice was carried out to allow the experimenter to try certain testing procedures and to find promising leads for further research. The total results obtained from 23 subjects were not significant. There were two main subjects, one of whom gave results with a probability of chance occurrence of less than one in 150 such series. A comparison of the score distributions of these two subjects suggests the importance of further work along these lines but with a more adequate experimental design. The experimenter recommends his method of recording all the faces of all the dice thrown, instead of just the hits.

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Over a period of five months, from December, 1948, to April, 1949, the writer carried out a preliminary PK experiment with six dice. While not in itself clearly demonstrative of the PK effect because of the design of the test (some of the results could conceivably have been produced by precognition), this experiment showed some interesting effects. The writer (Subject B) and his wife (Subject A) were the principal subjects. A total of twenty-three subjects participated, although some of these contributed only half a dozen runs each.

The series was designed to detect PK ability in the subjects and to allow the experimenter to try out testing techniques, conditions, and apparatus. The experiment was not well balanced since the subjects were allowed for the greater part of the series to select their target faces, and consequently there were not equal numbers of runs for each face of the die. That dice bias favoring the most frequently used targets appears not to have produced Subject A's significant result is shown later.

Procedure

Six 9/16-inch plastic dice were employed. These were released on an inclined plane at the bottom of which they struck a small horizontal projection and were scattered onto a felt-covered table.

During the course of a session the subject had no physical contact with the dice or apparatus since the dice were released by the withdrawal of a ruler on which they were supported and the releasing was done independently by an assistant who acted as "releaser." After each trial the releaser called the faces of all the dice and these were recorded by the experimenter-recorder. The subject had no part in the recording. All trials were witnessed.

The experimental procedure and scoring differed somewhat from the usual practices. Each subject completed one or more record sheets at a session. A sheet contained either one or two columns, each column representing 25 releases of six dice, so that a sheet was the equivalent of either 6¼ or 12½ standard runs of 24 die throws. The same target was used throughout a column of 25 throws. The results are interpreted in terms of standard runs.

The experiment was arbitrarily terminated when certain of the results suggested that it would be worth while to change to more rigid and well-balanced procedures.

So far as releasing and recording were concerned the conditions of the experiment were strict. However, the tests were carried out in the writer's home with visiting friends enlisted as subjects whenever the opportunity occurred. To this extent the atmosphere was casual and even social, and it is thought that this may have contributed in some degree to the results produced by the writer's wife, who is more than usually responsive to such conditions.

In general terms, the four principal subjects were keenly interested in the possibility that a PK effect might be demonstrated, while the remainder had comparatively little interest in the subject, were newly introduced to it, and were not entirely convinced of the possibility that mind might exercise any influence upon matter.

Results

Total Results

The total for all subjects was positive but not significantly so; the 1,356¼ runs of the entire experiment gave a positive deviation of +93 (CR = 1.41; P = .16). There were four subjects who completed in excess of 100 runs each. Subject A produced a striking positive result; so large a deviation as hers would be expected

as a chance occurrence only once in 150 such sets of throws. Of the results of the remaining three, one was positive and two were negative, all insignificant. The results of the main subjects and of the experiment as a whole are set forth in Table 1.

Table 1

TOTAL RESULTS

	Subject	Runs	Dev.	CR
A	L. M. R.	331¼	+83	(+)2.51
B	R. R.	393¾	+41	(+)1.14
C	D. A. M.	150	− 1	(−) .05
D	M. C.	106¼	−13	(−) .69
	Other subjects	375	−17	(−) .48
	All Subjects	1356¼	+93	(+)1.41

The only individual results that seemed extensive enough and of sufficient interest to justify further study were those of Subject A, the wife of the experimenter, and Subject B, the experimenter.

Subject A

Subject A produced 331¼ runs with a deviation of +83. These results at first suggested that a bias of the dice in favor of the six-face had produced the significant deviation, since this subject had chosen to throw for the six-face far more frequently than any of the other faces and had had relatively more success with it. However, comparison of the scores for the six-face when it was target and for the six-face when some other face was target strongly suggests that dice bias for this face is not the explanation of Subject A's results. (In fact, analysis of all results by this method strongly suggests a dice bias for the five-face.)

For the 125 runs which Subject A performed with the six-face as target there was a deviation of +85 with a CR = 4.16. The number of sixes which she threw in the remaining 206¼ runs of her data when sixes were not the target gave a deviation of −14 and a CR of (−) .54. The CR of the difference is 3.59.

With the other faces the difference is not nearly so marked, and the five-face is the only other outstanding one. An insignificant *negative* deviation occurred on that face when it was the target.

Table 2

CHRONOLOGICAL DISTRIBUTION OF HITS—SUBJECT A

	Runs	Dev.	CR
1st Quarter	83	+56	(+)3.37
2nd Quarter	83	+ 2	(+) .12
3rd Quarter	83	+29	(+)1.74
4th Quarter	82.25	− 4	(−) .24

The deviation for fives when not the target is significant (237½ runs with a deviation of +64 and a CR of 2.99). This result strongly suggested a dice bias favoring the five-face. (A possibility that should be borne in mind is that extraneous, misdirected PK produced this result. The writer had a particular preference for the five-face and usually released for Subject A.) However, this subject obtained her total result largely because of the six-face successes.

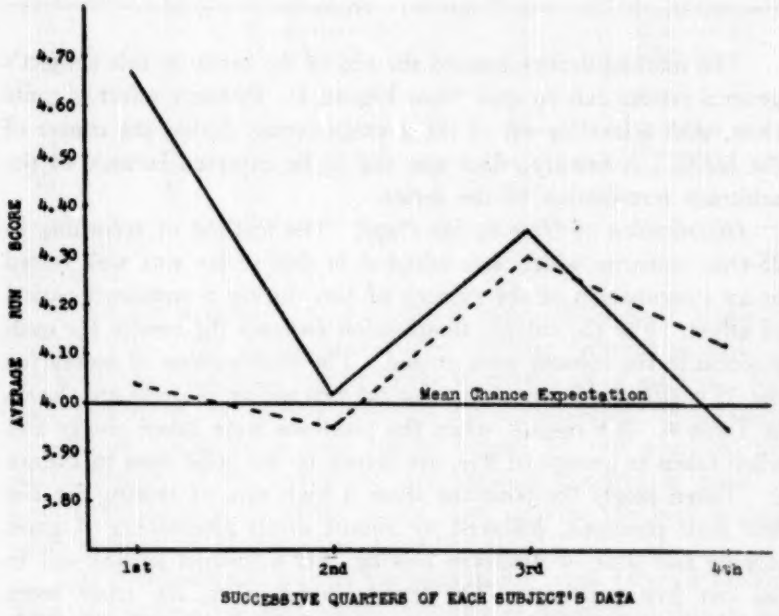

FIG. 1. Chronological distribution of scoring for Subject A (solid line) and Subject B (broken line).

Chronological Decline. There was a marked chronological decline in Subject A's scoring (see Table 2 and the solid line of Figure 1). To some extent this decline corresponded with the shift of attention from the six-face. Of the total number of times this face was the target, 84% occurred in the first quarter of the experiment, 5% in the second, 11% in the third, and none in the last. When the six-face alone was considered, there was no evidence of a decline (see Table 3) so it is not possible to say whether the general decline is a chronological or a target face effect.

Table 3

CHRONOLOGICAL DISTRIBUTION OF SIX-FACE HITS—SUBJECT A

	Runs	Dev.	CR
1st Quarter	31¼	+20	1.96
2nd Quarter	31¼	+20	1.96
3rd Quarter	31¼	+25	2.45
4th Quarter	31¼	+20	1.96

The marked decline toward the end of the series in this subject's general results can be seen from Figure 1. Primacy effect is quite clear, with a leveling-off of the average scores during the course of the series. A finality effect was not to be expected because of the arbitrary termination of the series.

Distribution of Hits on the Page. The method of recording in 25-trial columns which was adopted in this series was well suited to an examination of the pattern of hits during a sustained period of effort. For the column distribution analysis the results for each position in the column were pooled. The distributions of scores for the 25 positions of the column for the two major subjects are shown in Table 4. A's results, when the positions were taken singly and when taken in groups of five, are shown by the solid lines in Figure 2. Taken singly the positions show a high rate of scoring for the first four positions, followed by almost direct alternation of good scoring and poor or mediocre scoring, and a distinct leveling-off in the last five positions. The trend shown when the trials were grouped into fives is similar to that found in other PK series (**1, 2, 3**): high scoring in the first group, followed by a gradual decline through the other four groups.

Table 4

DISTRIBUTION OF HITS ON THE PAGE FOR SUBJECTS A AND B

Position of Trial in Column	SUBJECT A		SUBJECT B	
	Singly M. C. E. = 53	Grouped M. C. E. = 265	Singly M. C. E. = 63	Grouped M. C. E. = 315
1	61		66	
2	62	Hits = 303	69	Hits = 308
3	64	Dev. = +38	56	Dev. = − 7
4	63	CR = 2.33	55	CR = .39
5	53		62	
6	60		55	
7	51	Hits = 276	66	Hits = 302
8	65	Dev. = +11	53	Dev. = −13
9	51	CR = .67	71	CR = .80
10	49		57	
11	61		56	
12	54	Hits = 281	77	Hits = 326
13	51	Dev. = +16	66	Dev. = +11
14	63	CR = .99	63	CR = .62
15	52		64	
16	60		63	
17	48	Hits = 275	53	Hits = 313
18	60	Dev. = +10	74	Dev. = − 2
19	57	CR = .61	61	CR = .12
20	50		62	
21	54		86	
22	55	Hits = 273	59	Hits = 367
23	54	Dev. = + 8	70	Dev. = +52
24	52	CR = .49	69	CR = 2.93
25	58		83	

Subject B

I, Subject B, threw a total of 393¾ runs with a positive deviation of +41. My performance is in striking contrast to that of my wife, particularly in regard to the distributions of success. My improvement in the third quarter of the chronological distribution (see Table 5 and the broken line of Figure 1) just preceded my wife's decline. I did not show such striking changes in rates of scoring throughout the series as she; and whereas her best scoring came at the start of the series, mine did not.

The distribution of hits on the page in my work (see the broken lines of Figure 2) is even more markedly in contrast to that of my wife. My grouped pattern is, in fact, almost a mirror copy of hers.

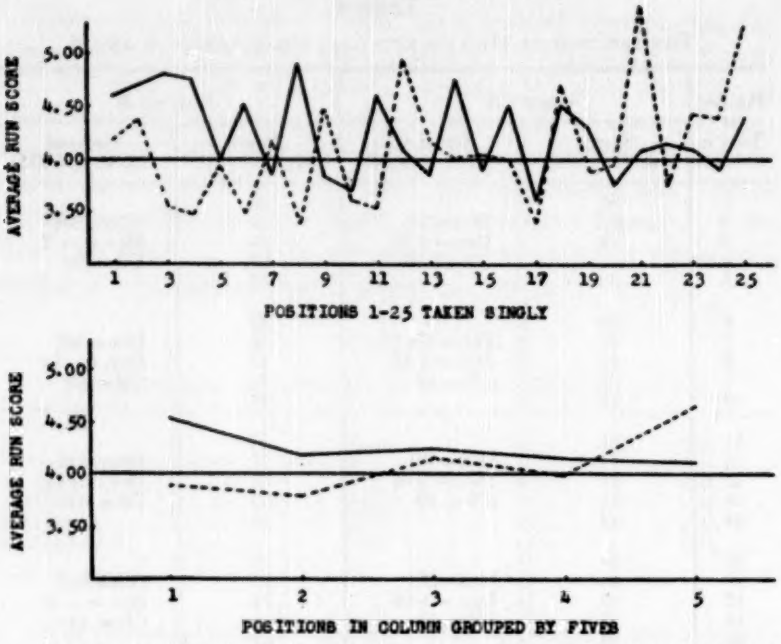

FIG. 2. Distribution of scoring in the pooled column for Subject A (solid line) and Subject B (broken line).

A test of the relation between the scores and the positions within the column, as shown in Table 4, was made for Subjects A and B individually. This was done in each case by obtaining the coefficient of regression of the position scores on the order of the throwing positions in the column. A *t*-value was then obtained for each of these regression coefficients. For Subject A, the regression coefficient was found to be −.2746, indicating that the scores showed this average

Table 5

CHRONOLOGICAL DISTRIBUTION OF HITS—SUBJECT B

	Runs	Dev.	CR
1st Quarter	98½	+ 4	(+) .22
2nd Quarter	98½	− 5	(−) .28
3rd Quarter	98½	+30	(+)1.66
4th Quarter	98¼	+12	(+) .66

decrease in going from one position in the column to the next, and the t is 2.01 (23 degrees of freedom; P = .06).

For Subject B, an average *increase* in score of .4954 was found in going from one position to the next, and the t is 2.17 (23 degrees of freedom; P = .05). The probabilities given are those for finding a linear trend toward either an increase or a decrease in scoring in the column where no relationship would be expected on the chance hypothesis. The trends observed are only suggestive, but they are sufficiently marked to justify further tests with these two subjects, especially as the conditions of the experiment rule out the possibility that biased dice or recording errors could have had any influence upon the result in question.

Control Series

At the conclusion of the experiment it was decided that an independent examination of the dice for bias was desirable. A control series was therefore carried out by Mr. G. F. K. Naylor.[1] It consisted of a total of 400 throws of the six dice (100 runs). The dice were thrown from a shaker onto a smooth carpet. Conditions similar to those commonly observed in PK experiments were applied throughout; that is, all dice were rethrown if any were "cocked." The results are shown in Table 6.

The distribution of the die faces falling uppermost strongly sug-

Table 6

Results of a Control Series of Twenty-four Hundred Die Throws

Face	Dev.	CR
1	−23	(−)1.26
2	− 5	(−) .27
3	−11	(−) .60
4	−14	(−) .77
5	+25	(+)1.36
6	+28	(+)1.53

[1] A control experiment by the writer or by anyone associated with the main experiment was ruled out as unsatisfactory since it was thought that the possibility of a PK influence, however remote, should be minimized. I am greatly indebted to Mr. G. F. K. Naylor, M.A., M.Sc., Dip.Ed., Senior Lecturer in Psychology at the University of Queensland, for carrying out this tedious control experiment, despite the demands on his time by a heavy research program. Mr. Naylor was aware that consideration had been given to a six-bias, but not that a bias for fives had been considered.

gests dice bias. Mr. Naylor has suggested (and I am entirely in agreement) that these results may have arisen "from the removal of material from the dice in the process of marking the faces More would be removed from the faces with large numbers and this would tend to displace the center of gravity in the opposite direction."

The possibility of dice bias for the six-face is consistent with the chronological distribution of hits for this face alone in the case of Subject A (see Table 3), but the significant difference between the score for sixes when they were the target and sixes when they were not the target remains a puzzling feature.

Discussion

Apart from suggesting the existence of some PK ability in Subject A and yielding results for Subject B that offer an interesting comparison, the principal value of the series was, first, in showing the advantages and the weaknesses in the method of scoring; and second, in suggesting specific aspects of the PK effect which warrant particular attention.

So far as method of scoring is concerned, it is thought that the recording of all faces was a tiresome procedure for the subjects and it might have lowered the scoring rate. On the other hand, the fact that all faces were recorded enabled a direct comparison to be made between the score on each face when used as target and when not used as target. The comparisons made possible by this information offset to some extent the unequal use of target faces and resulted in the detection of an apparent dice bias. The length of each page provided detailed information on hit patterns.

Despite the variations from standard practice the scoring trends were found to be essentially consistent with previous reports. In view of the differences between the two major subjects in scoring distributions, it is important to characterize their differences in attitude. Subject A is particularly interested in PK and its possible importance in other branches of learning, especially biology. She had a keen desire to produce significant results. On occasions, but not invariably, she declined to take part in a session if she felt that she could not produce positive results. Her attitude toward the tests from time to time, her physical state and health, appeared to be distinctly reflected in her scores. Her poorest page score (14,

when the mean chance expectation was 25) was recorded when she rose from bed to take part in the test during a severe attack of tonsilitis.

Unlike other subjects, who merely fixed their attention on the target face, she attempted to influence the falling dice with a real and perceptible effort of will. Often she called the target face aloud with determination. She claimed that when she produced good results visualization of the target face was largely responsible and that she was particularly aided in the case of the six-face by a curtain pattern in the room in which the tests were conducted.

Whereas my wife had begun the experiment with determination to produce a significant result, I, although enthusiastic, was not compelled by such ambition. I was undoubtedly influenced by her successes in the early part of the series and spurred to further efforts. In the early part of the series I experimented in different mental attitudes, endeavoring to determine introspectively a key to conscious control.

The results may perhaps be explained in terms of personality traits. Whereas my wife quickly adapts herself to new situations, I tend to meet emergencies unprepared; whereas she tends to be impetuous, I am over-cautious and slow. These observations, comparatively unimportant in themselves, indicate the desirability of comparing personality traits, as shown for instance by Rorschach and other personality tests, with experimental psi results.

The loose design of the experiment does not now warrant any more detailed examination of the effects, though these appear to justify further investigation. One such effect remains to be mentioned: it was found that subjects had a tendency to score well on the die face which had been the target on the previous occasion, regardless of whether they or someone else had been the subject. In fact, one subject (B) produced a better result on his own *preceding* target face than on the actual target face (381¼ runs; deviation, +69; CR = 1.62). The impression, whatever its value, could scarcely have been formed unless all faces had been recorded.

The practice of having an observer release the dice for the subject was first instituted as an additional safeguard for the security of the experimental conditions. It is doubtful if it served any useful

purpose from this point of view. There were different reactions by the subjects to this procedure which suggests that the matter might better be left to individual choice in future tests. Some subjects appeared to be more at ease and able to give more attention to the effort of concentrating on the target than they would have been able to do if employed in some way in the act of throwing or releasing the dice. Others appeared to feel a sense of isolation and would possibly have produced better results if their part in the experiment had been less restricted.

The series adequately fulfilled its objective in showing the advantages and disadvantages of the experimental approach used. While it is, in itself, not a specific contribution to the existing knowledge of PK, it is in part confirmatory of earlier findings and has provided useful data for the planning of future experiments.

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PARAPSYCHOLOGY AND MODERN SCIENCE

By C. C. Stevens

Dr. J. B. Rhine's book, *The Reach of the Mind*, was published in England in 1948, and the present writer has just had the pleasure of reading it. The information it gives on the researches carried out at Duke make fascinating reading, and all concerned are to be congratulated upon a thrilling and painstaking piece of work. To at least one reader the dice-throwing tests and the results obtained are brand new and suggest a wide range of exciting speculations.

Dr. Rhine is much concerned in his Chapter 10 about the reluctance of scientists in other fields to accept the implications he sees in psi phenomena. He diagnoses fear as the cause—fear of dualism and fear of losing caste or prestige. Although such fears will play their part in hindering some from accepting his findings, there may be other factors tending towards the same result, factors which are more susceptible of alteration than men's fears. From the writer's point of view it would seem that professional scientists will be more likely to look askance, not at the conduct of the experiments, but at the conclusions drawn from the data obtained and at the methods used in interpreting them, and that, in consequence, they may overlook the importance of the laboratory work that has been done.

Dr. Rhine writes: "*First*, . . . start out with a clearly stated problem, one . . . sharp and specific and free from unwarrantable assumptions."[1] This is indeed a fair description of modern scientific method and it is the last four words that comprise its most important part, *free from unwarrantable assumptions*, for the assumptions adopted largely control the manner in which the problem is stated and thus control the answers the experiment will provide.

Attention to underlying assumptions is fairly new in the world: In classical Greece, and for many centuries later, they were ignored—not merely taken for granted, but their presence never suspected. Euclid, for instance, assumed that only one line could be drawn through a point off a given line so as to be parallel to the given line,

[1] P. 158 (American edition).

and he was never able to prove it. About twenty-four centuries later Riemann said, "Well, suppose more than one line *can* be parallel. Let us take as one of our assumptions that you can have more than one." And on that basis he produced a complete geometry, just as self-consistent as Euclid's, but very different, a non-Euclidean geometry for which important applications have been found in the modern world. And incidentally he discovered that Euclid's assumption was a function of his definition of a plane surface, that when Euclid chose his plane he at the same time involved himself in his unprovable proposition.

We are born into this world knowing nothing about everything and we have so far found only one method of dealing with our situation, namely, we make some assumptions and try the results, ready to make different assumptions and try again if for any reason we are not fully satisfied with the results of the first. Any puppy or child can be seen doing this all the time in its early years. But the point that distinguishes twentieth-century science from earlier efforts is the recognition that: (1) No assumptions are compulsory, and any set can be chosen; (2) a change in even one assumption will affect the whole of the structure erected upon that set; and therefore (3) the assumptions used must be known to the experimenter or he will not be able to reap the benefit that may follow a deliberate change. It is not too much to say that the flowering of mathematics in the nineteenth century and the subsequent invigoration of scientific investigation in the twentieth have largely been brought about by the adoption of this technique.

Let us then examine some of the assumptions used in Dr. Rhine's book: "The brain, of course, lends itself to study by physical methods. . . . The mind on the other hand is intangible."[2] And further on: "Psychokinesis, after all, is just a new word for an old idea. The word is even in the dictionary: 'The action of the mind upon a physical system.' Belief in such a capacity of the mind, especially in its relation to the body, is probably as old as the distinction of mind and body itself."[3]

From these extracts, and from most of the chapter headings, it may be seen that one assumption used here is that a man is com-

[2] P. 5.
[3] P. 87.

posed of a body which is physical plus a mind which is not. It is now suggested that the relation between the existence of the term "mind and body" and the belief that mind is something not physical is similar to that obtaining between Euclid's plane and his unprovable proposition; that is, they are both aspects of the same underlying assumption and are contemporaneous in origin.

The preceding quotation continues: "It is one of those familiar, taken-for-granted concepts that are no longer noticed in our intellectual life."[4] In the history of science there have been many examples of taken-for-granted concepts proving a considerable handicap to advance. The concept of "heat" kept back the early physicists for generations. It had volume, they said, because when you put heat into something it expanded. But they also observed that the heated thing weighed no more, and they concluded that heat therefore must be something different from the rest of the world in having volume but no mass. This impasse effectively blocked further progress until it was resolved by a different formulation: "Heat" was no longer described as a "thing" but as a state of molecular activity. Yet the distinction between a hot article on the one hand and "its heat" on the other had been as old as the language. As soon as the old assumption had been replaced by a new one it became possible for modern thermodynamics to appear.

In this book, Professor Rhine considers many hypotheses but nowhere does he depart from his prime mind-plus-body assumption; he never questions, for confirmation or amendment, this way of thinking about man as made up of two pieces. When touching upon psychosomatic medicine, in which the effort is to treat the whole man without making prior assumptions about which is the most useful way of dividing up the whole into parts or subsystems, his assumption compels him to interpret psychosomatic practices as confirming the existence of "mind" and "body" as distinguishable entities.[5] Even when referring to modern psychology he still deals in terms of the dichotomy although those to whose work he refers are using a different set of assumptions which does not include this one. He says: "The student . . . finds nothing about the soul in modern psychology textbooks and lectures, and very little on the

[4] *Ibid.*
[5] P. 93 and p. 197.

mind as a distinct reality. Instead he studies about 'behavior' and its relation to brain fields and pathways. . . . Yet oddly enough no one even claims to have proved that the mind is physical."[6]

Those who study behavior in relation to brain structure have already produced most valuable results, but their work has not been based upon the age-old assumption of the existence of "mind" as a distinct reality. They have endeavored to observe the activity of the whole organism in its environment and as they are not using the concept of "mind" at all, the question of "proving the mind is physical" has not arisen since it is entirely irrelevant.

Another assumption underlies his analysis; it may be another facet of the one mentioned above and it is rather more hard to describe. It concerns the scope and function of physics. For instance, it is written in Chapter 4: "Now, if his identification of the cards had been made possible by a physical energy emanating from them, the increase of the distance between himself and the source of that energy would have been expected to reduce its effectiveness in proportion to the ratio of the squares of distance. . . . But he was more successful at the greater distance. What is known of the laws of physics governing energy transfer does not apply to these findings."[7] And in Chapter 7 there is the statement: "The finding that mass, number, and form are not determining conditions in the PK tests, takes its place then alongside the discovery that time and space were not limiting factors in ESP,"[8] from which it is concluded: "There can scarcely be any doubt that PK is nonphysical."[9]

The reasoning here, and in other similar passages, reflects an assumption about physics which is admittedly not very clear to this writer. It seems to be, perhaps, that the scope of physics is limited, among other ways, to energy exchanges which always vary with the square of the distance. If this were the case, if physics were indeed confined in this way, could the quantum theory ever have been evolved by physicists, dealing as it does with energy exchanges which noticeably do not vary in precisely *pro rata* degrees? To look at the same question from another angle, it has hitherto been generally agreed, roughly, that physics is concerned with observable phenomena in the world around us; in the Duke data we are presented with delicate and precise statistical measurements, the breath

[6] Pp. 6-7. [7] P. 55. [8] P. 117. [9] P. 116.

of life to the physicist. And yet the conclusion is offered that the phenomena are "nonphysical," simply because some relationships usually present in macrocosmic events have not been detected here. Scientists are already familiar with many instances where the ordinary rules of our daily life on the surface of this terrestrial globe do not apply to different scales of conditions (for example, the extraordinary behavior of some liquids within a few degrees of 0° K in defying gravitation or the inapplicability of our macrocosmic "cause-and-effect" formulation to nuclear radiation); and there will be many who, however unprejudiced in their acceptance of the Duke statistics, will not be able to regard as scientific this archaic treatment of the data.

There are other features of the interpretation placed in this book upon experimental results which do not accord with present trends of modern science. For an introduction to the manner in which the adoption of new and different assumptions can lead to a new and more effective approach, the interested student is referred to an article by Robin Skynner in *The Rationalist Annual* for 1949, which the writer has sent to the Duke Library.

These notes are written first to record great appreciation of the intensely interesting work done at Duke in this research, and secondly to suggest that there are still untapped benefits to be gained from a different approach. It is suggested that advantage would flow in two ways if the flexibility of modern scientific method were utilized more fully: (1) practitioners in other professional fields would be encouraged to make their own contributions; and (2) a review of the data in the light of different assumptions would reveal new interpretations and lead to new experiments and to further progress.

The influence of the two assumptions adopted, that mind exists as an intangible but distinct reality, and that there is some class of measurable phenomena in the universe which are nevertheless beyond the range of the physical scientist, can be seen throughout the book. For instance, the question is asked: "Is there anything extraphysical or spiritual in human personality?" to which is added: "The experimental answer is yes. There is now evidence that such an extraphysical factor exists in man."[10] A fuller answer, on the evidence provided by the Duke research, would be: "If the existence of 'mind as a distinct reality' is postulated *a priori*, the data give no

[10] P. 206.

evidence against such an assumption; if, however, such an assumption is not made (as in psychosomatic medicine, biology, and other sciences), then the data cannot be interpreted as providing any evidence, either for or against, the suggestion made in the question."

No implication is being made here that the "mind-plus-body" assumption is *wrong*, nor can it be any more "right" than another; the question is merely: will any other assumption get us any further? Instead of assuming, as hitherto, that it is necessary or useful to think of man as made up of two parts, mind and body, a different assumption could be made, such as that it may be useful to prevent the importation of any such preconceived ideas by thinking and talking about man as a dynamic organism living in an environment.

What difference would this make to the interpretation of the psi data? In the first place, the dice work results would disclose the existence in some circumstances of a relation between a man and some part of his environment, formulated by people like us (but not necessarily by *all* men) as "objects," and this relation would appear to be of a type which, although mentioned often enough colloquially, has not received sufficient study by physiologists and others for any serious theory to have been put forward. But the results would not, on this basis, be taken as proving the existence of a "mind," for the data provide no evidence as to which part or parts of the subject have been concerned. As a result of previous cultural conditioning the subject may sincerely believe he is using his "will," but as far as the evidence goes it might just as well be his pituitary gland. The insoluble "puzzle" as to "interaction" between body and mind turns out to have been of our own making and would no longer arise to irk us—we would deal with the function of the organism as a whole.

The purpose of this article is to say to those concerned in the work at Duke: "Taste and try," to indicate that there are possibilities open which, as yet untried, promise exciting rewards. But the rewards can be grasped only by those who will take the trouble to acquaint themselves with, and train themselves in, modern methods of evaluation.

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[When Mr. Stevens presented Dr. Rhine with a copy of the preceding article for his comments, there developed a correspondence in which the two authors attempted to clarify their respective points of view. It seemed to the Editors that this correspondence made a worth-while contribution to the discussion, and permission was obtained to publish the following revelant comments from the letters. —Ed.]

J.B.R. to C.C.S.: June 24, 1949

. . . In passing over my conclusions regarding the nonphysical nature of the psi processes, you lodge only on the point that physics is not limited to the inverse square law. How do you propose to explain the results of the precognition tests?

I think there is much profit in a good critical pointing-up of the limitations of our present thinking on these matters. It would be very helpful to us if you could take into account these comments of mine and go on to show us where we are still inadequate in our conceptions of the results.

C.C.S. to J.B.R.: October 11, 1949

. . . So far no one seems to have consciously applied modern postulational method to this field (the method faintly outlined on page 128). The Victorian ideas of "discovering (eternal) Laws" and related notions are the latest that psi investigators have apparently heard of. I venture to predict that, until psi workers learn and apply the postulational method, (1) they will continue to tie themselves in knots of their own manufacture (for example, Thouless and Wiesner);[11] and (2) no modern scientist will in his professional capacity pay attention to or make any contribution to the study.

In writing the above I am not overlooking your article, "Impatience with the Scientific Method in Parapsychology," where you say, "What other methods and approaches are there?"[12] implying that there are none. My article might be regarded as supplying a more productive answer. In the JOURNAL of March, 1948, Tyrrell refers to the "need to . . . expand our ideas to fit the facts and not to try to draw the facts into the orbit of our existing ideas."[13] I endorse this

[11] "The Psi Processes in Normal and 'Paranormal' Psychology." *Journal of Parapsychology*, September, 1948.
[12] *Journal of Parapsychology*, December, 1947, p. 289.
[13] "Parapsychology: Position, Program, Outlook," *Journal of Parapsychology*, March, 1948, p. 41.

sentiment fully, but I have to observe that parapsychologists are using the old, traditional methods, those same ones which have been surpassed by the new in all other fields where they have been applied.

About your reference to my "lodging only on . . . physics . . . not limited to the inverse square law," I would quote your own remark in "Impatience with the Scientific Method": "Anything that produces effects of any kind thereby gives us something to observe and study"[14] and the remark of Dr. Alan Gregg (quoted on the back of the December, 1948, JOURNAL): "I plead with you because I believe that the greatest handicap to the study of human beings is unconscious preconceived notions."[15] The point I wish to stress is that the whole of your "conclusions" about "nonphysical minds," "dualism," "interaction" between "*the* two systems," and so forth depend entirely for their validity, and for their very existence, upon the prior (unconscious) assumption that man comprises two pieces, mind and body. This assumption is by no means a necessary one: there are no necessary assumptions. If it is not adopted and if another, different, one is taken in its place, not one of your conclusions follows from the data. . . .

J.B.R. to C.C.S.: December 22, 1949

. . . You did not answer my question as to how you propose to explain the results of the precognition tests. I did not understand your quotation of my remark, "Anything that produces effects of any kind thereby gives us something to observe and study," as an answer to that question, as you evidently intended it to be. . . . In my experience in discussing our findings with scores of physicists and in talking to groups in which there were hundreds present, I have not run into anyone yet from that profession who takes issue with the necessity of going outside physical theories to account for these results of precognition experiments. You may be a physicist, and if so, you will be my first exception. I have, however, found quite a number of psychologists and other nonphysicists who are so loath to give up a physical monism that they will more easily reject the findings themselves.

[14] *Op. cit.*, p. 285.

[15] "The Profession of Psychology as Seen by a Doctor of Medicine," *The American Psychologist*, September, 1948, p. 400.

I want very much to have the stiffest kind of analysis of my interpretation of our results with respect to physics. . . . The question is: Do the results require the hypothesis of a nonphysical property of the individual? I wish you would try to show that they do not. Try to force them into the current conception of the physical universe. . . .

C.C.S. to J.B.R.: January 17, 1950

. . . This passage [the final paragraph of the preceding letter] indicates that the crux of our failing to see eye to eye lies in the ideas we respectively hold about physics or science.

Science, like all human activities, is dynamic, developing, constantly changing. I will use nomenclature of the form: science $_{1600}$, science $_{1800}$, science $_{1950}$, to stress the difference between different stages. This is not to be taken as suggesting that at a given date all scientists hold the same views. The leaders of scientific advance are always ahead of their contemporaries. Their innovations do not become generally absorbed until included in students' textbooks; and in consequence, especially perhaps in this century when deeper revisions than ever before are being made, most professional scientists are twenty years or more behind the leaders. In view of this it is no surprise that the numerous scientists you mention who may perhaps be said to be versed in science $_{1900\text{-}1930}$ have not brought to your attention the applications of science $_{1950}$. Their comparative inflexibility, which you yourself have noticed, is also a feature of science $_{\text{pre-}1930}$ in many cases.

For a full grasp of the difference between science $_{1900}$ and science $_{1950}$ a somewhat thorough training is required and for this reason I cannot hope to "explain" the difference in the course of a letter and did not try to do so in my article. But one aspect of the difference is made evident in the passage quoted above by its demonstration that the ideas about physics which you are using diverge in at least one important particular from those of science $_{1950}$.

Your attitude leads you to say, if I may paraphrase rather freely: "Here on my left hand we have a collection of observations about energy, mass, inertia, space-time, and so on. These are accounted

for by physics. Physics is the making of observations about the phenomena of the physical world and accounting for them by theories, hypotheses. On my right hand, however, I have a collection of observations which are not accounted for by physics. They cannot be forced into the current conception of the physical universe. Therefore, (x) these are nonphysical. They require the hypothesis of nonphysical properties."

It is at the point marked (x) that the effect of your assumptions (that is, postulates) comes in. The questions you asked were all framed against a certain background. "Is there anything extraphysical about human personality?" can only make sense when asked about a universe in which the existence of something "extraphysical" has been previously postulated as a possibility; that is, it is a dualistic question and cannot but beget a dualistic answer. The dualistic character of your background is also clearly indicated by the statement I have quoted from the second page of your book, "brain . . . physical, mind . . . intangible," and by many others. Having in this way obtained apparent "confirmation" of your postulate, the rest follows as in the above paraphrase.

Let me now put the position in more appropriate terms: "On my left we have a collection of observations about energy, inertia, space-time, and so on. These and theories as to their interrelations comprise physics $_{1950}$. On my right hand, however, I have a collection of observations that are not accounted for by the theories of physics $_{1950}$."

What is the next step? Now, this is no new situation. It has been exactly paralleled innumerable times in the past. For example, Newton's theories accounted well enough for the observations made up to his day, but were not adequate for subsequently accumulated observations, say of the motion of the perihelion of Mercury. What happened in that instance? Men could have said: "This is not accounted for by physics $_{1679}$. This cannot be forced into the current conception of the physical universe. Therefore it is proof of something nonphysical. It requires the hypothesis of nonphysical properties." Such an interpretation could have been put upon the situation but it would have been more like science $_{500\ \text{B.C.}}$ than

science $_{1600}$ and is entirely incompatible with science $_{1950}$. And I suggest that there is no difference in structure between such imaginary interpretation and that which you have adopted. . . .

It is still important that awkward facts be faced and kept to the fore, as pointers to the next direction in which revision is required. This link is not forged if they are treated in an unscientific manner and relegated to an "extraphysical" limbo where the professional physicist can easily ignore them.

. . . It has no adverse effect on my case that I have no answer to your question, "How do you propose to explain the results of the precognition tests?" It is part of my case that there is no answer at present to this question and that it is therefore of immense importance that the professional physicists be inescapably confronted with it in inexorable form. This requires that the whole investigation be conducted in the manner of science $_{1950}$, which includes rigorous and trained use of postulational method and which is conspicuous by its absence so far.

J.B.R. to C.C.S.: February 3, 1950

I take it that your ground for assuming that I began with a dualistic bias is that I raised the question regarding nonphysical properties in personality. . . . It was, of course, the appearance of what seemed like unexplainable (physically) phenomena that raised the question whether there are relatively fundamental distinctions to be made such as dualists in the past have attempted to make. I do not see how this approach implies a dualistic bias. . . .

I would be very much concerned if you were correct in your impression that by calling psi nonphysical it would lose the interest of physicists. I am rather inclined to think the opposite way; namely, that by drawing a line that bounds physics we attract the attention of physicists to something physics has not known before, and something, therefore, new and challenging. . . .

C.C.S. to J.B.R.: February 15, 1950

. . . My letter of January 17 tends to suggest that I am merely concerned with whether psi phenomena should be classified as "unexplained physics" or "extra-physics." In my view, that question is a minor detail.

In your letter of February 3 you write, "Your position as I now see it is similar to . . . the physicist (who) would prefer to suppose that the physics of tomorrow will be able to deal with those odd phenomena." I am not content with this passive attitude. The development of physics or parapsychology or any other human activity is not an automatic progression; it depends on the *methods* used by those concerned with it. My intention is to stress the need for psi workers to hear of, to learn and acquire, and then apply to their field the full evaluationary technique of modern postulational method. When some of them do so, it will be they who, in the words of your letter under reply, will be "liberated from restriction on their thinking" and not those who continue to employ the stultifying methods of tradition.

J.B.R. to C.C.S.: February 21, 1950

It occurs to me that we might get a further bit of clarification of your recommendations if I ask you to explain what you mean by "the full evaluationary technique of modern postulational method." Your use of "evaluationary" and "postulational" in the same expression seems to me to be in need of justification. I, myself, have become convinced that the use of postulates in scientific method represents a carry-over of the prescientific or philosophic method of attempting to solve problems; that is, in dealing with the real world as distinguished from mathematics, it is better scientific procedure, I am convinced, to attack the questions that are concerned in the postulates before going on to the question for which the postulates are made, just the same as it is good principle to build a foundation first in constructing a house. Even more strongly I would urge that, if one is to take something for granted in the form of postulates, for the sake of argument or out of impatience to get on to some sort of a solution, he certainly ought not to use the word "evaluation," but might conceivably regard his technique as exploratory.

Scientific method has necessarily been of greatest concern to us in our precarious field of exploration, and we have gone far enough to recognize that it would not be simple to clear up any differences we may have on this matter in a short space. I merely want to go on record as saying that I do not know of any modern postulational

method that seems to me properly scientific. I can, however, join you in your emphasis of the importance of an active rather than a passive attitude which we hope the parapsychologist will take in the matter of looking for hypotheses relevant to the answering of his questions. I will gladly join you in insisting that any hypothesis that can be borrowed from modern physics should be as readily applied as those from any other field. Now my whole point is that I insist merely that we be on the lookout for hypotheses that are not physical in type, and that they be not bound by physicalistic thinking, because the phenomena, as far as they have been studied, do not conform to a physical pattern of description.

Some day somebody will doubtless discover the interconvertibility of energies involved in the systems we are talking about, as eighteenth-century physicists did with work, and heat, and some other categories. But for the present, it seems to me that it is important to emphasize the divergence of concepts to which parapsychological findings have led us, distinctions to be made within the personality of man, and the relative dualism involved in his makeup. In so doing we achieve a better understanding of the behavior of man as we already know it and we keep all the ways open to further advances in knowledge, allowing for a freedom from physical causation which would rest on a pretty thin argument without some relative dualism.

C.C.S. to J.B.R.: March 10, 1950

There is nothing inherently incongruous in postulational evaluation; indeed, no method of evaluation exists that is not founded on assumptions.

In the article by Robin Skynner referred to above there are some clues that those interested may follow up, but I agree that no concise exposition of modern postulational method is at present extant. I am hoping this will be remedied later on in the year by the publication of my book on the subject.

A COMPARISON OF NAMES AND SYMBOLS IN A DISTANCE ESP TEST

By Morris Skibinsky

ABSTRACT: This experiment was designed to test the hypothesis that subjects will score higher on ESP targets which are psychologically very meaningful than on targets which have little or no personal significance. The experimenter compared ESP scores of six subjects in three series of GESP tests when family names were used as targets and when ESP symbols were used as targets. It was expected that the names would score higher than the symbols. Three of the subjects were friends of the experimenter and three were members of his immediate family. The experimenter, who acted as sender, was located in Durham, North Carolina; and the six subjects, who acted as receivers, were in New York City.

A total of 600 runs were done, and the total scores were not significantly different from chance. Further analysis showed, however, that in all three series and for all six subjects name targets scored lower than ESP symbols. The average score for the name targets was 4.72; the average score for the ESP symbols was 5.16. Such a difference or one greater would occur by chance about once in 143 such experiments. The greater part of this difference was contributed by relatives.

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Introduction

The assumption that the world in which we live registers upon us all in about the same way has fallen into disrepute among psychologists. The contemporary point of view (based upon extensive work in clinical psychology, use of the Rorschach and other projective tests, etc.) holds that perception is a function both of the individual perceiver and of the stimuli which elicit his observations. Individual differences among perceivers will account for the variety of interpretations which may be given by different observers to some fixed set of stimuli. In general, it is literally true that there will be as many ways of "seeing a situation" as there are people to see it.

It is to be noted, however, that stimuli may be arranged in an orderly sequence with respect to the degree of variation in interpretation which their physical natures will allow. Some types of stimuli, that is to say, lend themselves more readily to subjective interpretation than do others. Thus, there are far-reaching differ-

ences, insofar as interpretation is concerned, between a rigidly constructed stimulus (a solid object like a desk), and one which is vague and unstructured (such as an ink blot). The former, as perceived by most people, will receive a common interpretation. The latter, as is well known, will be much less likely to draw the same interpretation from different individuals. It would appear, then, that patterns of stimuli in the sequence mentioned above, form a continuum. At one extreme lie those patterns, the interpretation of which rests almost entirely with the observer, while at the other, those whose rigidity of structure leaves little doubt as to what has been perceived.

It has occurred to the writer that stimuli may similarly be arranged in a continuum in terms of their ability to elicit recognizable psi phenomena. Here, by hypothesis, it is the *psychological,* rather than the physical nature of stimulus patterns which causes them to be rigidly or loosely structured insofar as their interpretation is concerned. To illustrate, there is a great accumulation of observations (contributed chiefly by studies of spontaneous psi phenomena) that suggest that when sufficiently strong factors are operating, such as the death of a close relative, an individual may suddenly become aware of information connected with the event. Sometimes with some people this information apparently brooks no opposition and, brushing aside all mental obstacles, enters consciousness. This type of stimulus pattern would lie, for the psi process, at an extreme of the just-mentioned continuum comparable to the extreme position of the rigidly constructed stimulus pattern in sensory perception. Both are sharp and clearly defined, allowing little freedom of interpretation or reconstruction by the perceiver. At the other extreme would lie those patterns whose psychological natures, like the physical nature of the Rorschach, lack sufficient forcefulness to prevent their being distorted by the inner needs and tensions of the individual.

Practical considerations and limitations exclude the possibility of using real life situations (such as the death of a relative) in order to test the above hypothesis. In casting about for some means whereby such a test could be carried out, it appeared to the writer that a modest beginning might be made by substituting proper names for the ESP symbols commonly used in ordinary testing. To use

proper names as targets (for example, in the usual GESP test procedure with sender and one or more receivers) would be to select test material psychologically more significant to subjects than the ESP symbols. Furthermore, if the proper names so employed were those belonging to the participants in the test, their use might assure stronger psychological and emotional involvement in the experiment on the part of these subjects than would ordinarily occur. Finally, let the participants belong to a family closely knit by emotional and other ties, and probably each proper name used would carry a maximum of psychological and emotional weight with each of the subjects. Thus, on the assumption that names would be somewhat representative of one end of the above continuum (rigidly constructed so far as interpretation is concerned) and symbols somewhat representative of the other end, the experimenter was led to expect a larger positive deviation to be obtained from tests involving names than from tests involving ESP symbols. With this expectation in mind, the writer arranged and carried out the experiments described below. The nature of the experimental design called for the comparison of scoring rates on the two types of target, to be evaluated by means of a test of significance of the difference between the two.

Problem

Will scoring in a GESP test situation be influenced by the psychological nature of the targets used?

More specifically, as related to the present study:

A. How will the use of proper names (belonging to members of the receivers' and sender's family) compare with the use of ESP symbols in a GESP distance test, in terms of evidence for the occurrence of psi? Will the rate of success when names are used be significantly greater than when symbols are used?

B. How will the effect of using names as ESP stimuli in the close personal family relationship compare with the use of names of no special significance to the subject?

Subjects, Material, and Procedure

The experiment to be described consisted of three series. Subjects were selected from the experimenter's family and from among

his personal friends. The family members were the experimenter's sisters: Betty, Yetta, and Eva. In Series I, two non-family members took part. They were friends of the experimenter, Mr. J.Z. and Mrs. P.H. These five subjects and a third, non-family receiver, Mr. E.M., also took part in Series II and III. Mrs. P.H. was unable, for personal reasons, to continue with the tests after the second day of the third series, and she notified the experimenter at that time to this effect. The subjects all lived in separate residences in New York City. Since family members had little more than passing acquaintanceship with the experimenter's friends, there was little communication between the two groups.

All three series were conducted with the subjects, as receivers, in New York, and the experimenter, as sender, at the Parapsychology Laboratory in Durham, North Carolina.

The targets for each run were written down on a mimeographed form with 25 spaces arranged in a square of five columns and five rows. Each target matrix was on a separate piece of paper, and two were used for each experimental session. In each cell of the first of these was written one of the following names: Betty, Yetta, Eva, Morris, Papa. Three of these names belong to the sisters who were receivers in the experiment, one to their brother, the experimenter, and one to the father of the family, who did not take part. In each cell of the second matrix was written one of the regular ESP symbols. To insure a random selection of the targets for each of the cells, Tippett's tables (3) were employed during the first two series,[1] and a table of random digits issued by the Interstate Commerce Commission (4), during the third. All targets were prepared the same day they were used.

Each series lasted four weeks and consisted of twenty individual target sets. (A target set consisted of one square of twenty-five names and one square of twenty-five symbols arranged as indicated.) The schedule was such that tests were conducted each night of the week from Monday through Friday, leaving weekends free. The first series began on October 11, 1948, and was completed Novem-

[1] On five days during the first series, the tables were not available. On those days, the targets were prepared with the aid of a numbered deck of cards. A name or symbol was assigned to each numbered card. The deck was thoroughly shuffled and randomly cut (with the aid of dice). The final card order was then used to fill out the target. Thus on these days, the subjects were guessing at a "closed deck," i.e., five names (five symbols) of each kind on the target sheet.

ber 5, 1948. The second ran from February 7 to March 4, 1949; the third, from October 17 to November 11, 1949.

The following is an excerpt from the instructions sent to each subject:

> Your end of the experiment will be as follows. Each night after the experiment has begun, just before you go to bed, sit down somewhere (by yourself preferably). Get in your mind the idea that I have been, or will be, sitting down in my room, at my desk (the very same day), and looking at the arrangement of names written out on a paper before me, and the arrangement of symbols written out on another paper before me. Then, without making any special effort at concentration, write down the names as you think they might be on the paper in front of me. Don't make any effort to get a particular name written down a specific number of times. When that is completed, write down the symbols as you think they might be on the other paper in front of me. In other words, what you will be doing is trying to get the names in the same positions in the five columns and five rows as they are on the paper in front of me, and you will also be trying to do that for the symbols.

During the first two weeks of Series I, the subjects' responses were written out on blank postcards. Target names were written out in full, by the experimenter on the target sheet and by subjects on the response cards. Subjects were instructed to alternate the position of names and symbols from night to night; that is, to write out names first the first night, and symbols first the second night, etc. At the beginning of the third week the procedure was simplified. Blank response cards were replaced by cards bearing two mimeographed five-by-five matrices for the subjects' guesses, the matrix to the left for names, the matrix to the right for symbols. Concurrently, the writing out of the names was discontinued (on both targets and response cards) and, instead, the initial of each name was substituted. These changes were maintained during the second and third series.

Subjects were instructed to write in ink and to date all cards.

All data were double checked, first by the experimenter, and then independently by members of the Laboratory staff.

Evaluation of Results

The fact that more than one individual attempted to duplicate each target that was set up complicates somewhat the evaluation of

the experimental results. The methods ordinarily applied in ESP work to determine the significance of differences are not valid here, unless it be assumed that the guesses of different subjects for the same target are independent of one another. There does not appear to be any procedural rule which will invariably ensure such independence, since it is always conceivable that some aspect of the test situation common to all subjects might cause individual guessing patterns to be related. Critical results in this paper have been evaluated both with and without the assumption of independence between subjects. A description of procedures employed may be found in the appendix to this paper.

Results

A total of 600 runs were completed by the subjects in all three series combined. Failure to send in cards eliminated 38 runs during the three series. Improper filling-out of cards (for example, filling in only four columns) occurred only during the first two weeks of Series I and resulted in the loss of six runs.

The total score on the 600 runs of the combined series did not deviate significantly from chance expectancy. The 299 runs in which names were used as targets show a deviation of −83 from the most probable number of hits if chance alone were involved in the results. The 301 control runs in which standard ESP symbols were used show a deviation of +49. The average score per run in each category, respectively, is 4.72 and 5.16. The critical ratio of the difference between these mean scores is 2.70. A ratio of this size or larger has a probability of occurrence by chance alone of .007. All six of the subjects showed lower total scores on the names than on the symbols. This difference in the results of the runs with names and symbols makes the chance hypothesis difficult to maintain.

In all three series the symbols gave a higher total score than the names, but there were interesting variations from series to series in the comparison of relatives and non-relatives.

In Series I, the major difference between the name and symbol categories occurred among the relatives of the sender, almost no difference at all being present in the scores of the non-relatives. In Series II, the major difference in scoring between the two target categories occurred among the non-relatives. However, it may be

Table 1

GENERAL RESULTS

SERIES I

Stimuli	Relatives			Non-Relatives			Total		
	No. of Runs	Av. Score	CR_d	No. of Runs	Av. Score	CR_d	No. of Runs	Av. Score	CR_d
Names....	52	4.52	2.33	40	5.20	0.10	92	4.82	1.68 (1.72)*
Symbols...	54	5.43		40	5.15		94	5.31	

SERIES II

Stimuli	Relatives			Non-Relatives			Total		
	No. of Runs	Av. Score	CR_d	No. of Runs	Av. Score	CR_d	No. of Runs	Av. Score	CR_d
Names....	56	4.63	1.04	56	4.70	1.79	112	4.66	2.00 (2.01)*
Symbols...	56	5.02		56	5.38		112	5.20	

SERIES III

Stimuli	Relatives			Non-Relatives			Total		
	No. of Runs	Av. Score	CR_d	No. of Runs	Av. Score	CR_d	No. of Runs	Av. Score	CR_d
Names....	54	4.65	1.15	41	4.78	0.11	95	4.71	0.94
Symbols...	54	5.09		41	4.83		95	4.98	

SERIES II AND III COMBINED

Stimuli	Relatives			Non-Relatives			Total		
	No. of Runs	Av. Score	CR_d	No. of Runs	Av. Score	CR_d	No. of Runs	Av. Score	CR_d
Names....	110	4.64	1.55	97	4.73	1.44	207	4.68	2.11 (2.10)*
Symbols...	110	5.06		97	5.14		207	5.10	

SERIES I, II, AND III COMBINED

Stimuli	Relatives			Non-Relatives			Total		
	No. of Runs	Av. Score	CR_d	No. of Runs	Av. Score	CR_d	No. of Runs	Av. Score	CR_d
Names....	162	4.60	2.61	137	4.87	1.15	299	4.72	2.69 (2.70)*
Symbols...	164	5.18		137	5.15		301	5.16	

* CR's of the difference when the subjects' attempted duplications of the same target-square are *not* assumed to be independent.

observed that the magnitude of negative scoring on the name runs was greater for the relatives. For Series III, as in the first one, the relatives accounted for most of the difference.

In Table I, Series II and III are shown combined because they were planned in the light of the results of Series I and are therefore of interest as an independent test of the relation between the two target categories. Since the scores in Series I were in the opposite direction to that expected, it was predicted that in the following series the total scores would tend to be lower when the names were targets than when the symbols were targets (see Discussion). A difference in the predicted direction between names and symbols as large as or larger than that observed in Series II and III would be expected as a chance occurrence about once in 58 such series (CR = 2.11).

In each of the three series, then, the name targets produced lower total scores than the symbol targets. A study of the direction of deviation of scores by individual subjects shows that they followed this general pattern with a fair degree of consistency in all three series (Table 2), though the χ^2 of 3.17 (1 d.f.) for the total result by this analysis is only suggestive (P = .075).

Response cards were checked against targets of the day previous to and the day following the one for which they were intended. Scores obtained in this manner were termed, respectively, backward and forward displacement scores. It will be observed that this type of analysis reduces the number of runs by 2 for each subject in each series. The more conventional trial-to-trial displacement analysis

Table 2

NUMBER OF SUBJECTS SHOWING POSITIVE AND NEGATIVE DEVIATIONS FOR THE TWO TYPES OF STIMULI

Stimuli	Series I		Series II		Series III		Total for Three Series	
	− Dev.	+ Dev.	− Dev.	+ Dev.	− Dev.	+ Dev.	− Dev.	+ Dev.
Names....	4	1	4	2	4	1	12	4
Symbols...	2	3	1	4	3	3	6	10
							$\chi^2 = 3.17$ (with Yates's correction) P = .075	

Table 3

FORWARD DISPLACEMENT

SERIES I

Stimuli	RELATIVES		NON-RELATIVES		TOTAL		
	No. of Runs	Av. Score	No. of Runs	Av. Score	No. of Runs	Av. Score	CR
Names	49	4.63	38	4.47	87	4.56	−2.04
Symbols	51	4.33	38	5.13	89	4.67	−1.54
Combined	100	4.48	76	4.80	176	4.62	−2.53

SERIES II

Stimuli	RELATIVES		NON-RELATIVES		TOTAL		
	No. of Runs	Av. Score	No. of Runs	Av. Score	No. of Runs	Av. Score	CR
Names	53	4.98	53	5.49	106	5.24	1.21
Symbols	53	5.45	53	5.17	106	5.31	1.60
Combined	106	5.22	106	5.33	212	5.27	1.99

SERIES III

Stimuli	RELATIVES		NON-RELATIVES		TOTAL		
	No. of Runs	Av. Score	No. of Runs	Av. Score	No. of Runs	Av. Score	CR
Names	51	4.59	39	4.87	90	4.71	−1.37
Symbols	51	5.08	39	4.92	90	5.01	+0.05
Combined	102	4.83	78	4.90	180	4.86	−0.93

could not be made as the experimenter could not be certain that subjects followed any particular pattern in filling in their guesses on the response cards. The day-to-day displacement analysis served as a convenient empirical check on the difference between names and symbols. Independence between the attempted duplications of the same target-squares was assumed throughout this analysis.

Backward displacement nowhere showed greater-than-chance deviations. Forward displacement scores are of interest because of

the results obtained in the first series. In this series, the 87 name runs showed a forward displacement deviation of −38; the 89 symbol runs, a deviation of −29. In all, the total forward displacement deviation for the 176 runs of the first series was −67. This deviation has a CR of 2.53. By far the largest portion of this deviation was contributed by the relatives (see Table 3 below). The second series reversed the trend of the first. Here 106 name runs showed a deviation of +25; 106 symbol runs, a deviation of +33; the total 212 runs of the series, a deviation of +58. The larger part of this deviation was contributed by the non-relatives. The third series followed the same pattern of forward displacement results as the first, but on a smaller scale: 90 name runs, −26; 90 symbol runs, +1; total deviation on 180 runs, −25. Again, as in the first series, the larger part of the deviation was contributed by the relatives.

The probability values of the CR's in these tables are not directly evaluable since no predictions concerning displacement scoring were made.

Correlation coefficients obtained between daily scores in the name and symbol categories indicate no tendency towards concomitant variations. Likewise no such tendency is revealed between name and symbol run scores on forward displacement nor on backward displacement.

Discussion

In examining the results of Series I, it was observed by the experimenter that when non-family members were omitted and only relatives were considered, the difference between names and symbols (though opposite to prediction) was large enough to be of interest. This (together with the large negative deviation obtained in the forward displacement analysis) led to the planning of the second series as a check upon the first. At that time the prediction was made, on the basis of the first series, that the total scores would tend to be lower when the targets being guessed consisted of names than when they consisted of symbols. This prediction was borne out. A third series was performed (prediction being the same as for Series II), and again the prediction was borne out. Differences obtained in the second and third series, when combined, are statistically significant at the 5% level. When the results of the three

series are combined the total difference between the name and symbol categories is of an *absolute* magnitude such as would be expected to occur by chance in less than one out of one hundred such samples.[2]

Obviously, the most important question to be raised concerning this work is the following: How is the difference between mean scores in the name and symbol categories to be accounted for? There are two obvious answers: chance variation or psi. Other possibilities are eliminated by the design and conduct of the experiment. It is admitted that a case for the existence of psi phenomena in general cannot be based upon the evidence here presented. Clarification of the problem is to be brought about only through further experimentation. It is the belief of the experimenter that the interests of this work will best be served by as near a direct repetition of the series above outlined as is possible. The establishment of the effect as one of extrachance character must of necessity precede serious consideration of its nature. Nevertheless, it is apparent that the results obtained fall into a range that is highly suggestive of possibilities for fruitful research in the future. It is intended that the following short discussion clear the ground for such endeavor.

Tentatively accepting the results as indicative of psi, we may profitably turn our attention to the reformulation of the original hypothesis. In essence, the basic hypothesis entertained by the experimenter in this work was that a subject's ESP performance in psi tests of the type outlined is to a certain degree dependent upon the psychological nature of the target material employed. Although the direction of scoring in the above experiment was opposite to that originally expected, the significant difference obtained between the two types of runs makes the hypothesis still applicable. It may be that target material which acts as a stimulus to the production of personal associations within the individual being tested tends to cause negative scoring in tests of the type here employed. No effort will at this time be made to define the term "personal associations" in any exact sense. The term is an attempt to express the psychological quality possessed by the target names used, under the assumption that it was the psychological nature of the target names which influenced scoring in a negative direction.

[2] Some margin of error is introduced into the evaluation of combined results in this experiment, however, because of the fact that the number of series (three) to be performed was not fixed in advance.

A possible alternate hypothesis (still assuming the results to be indicative of psi) which may account for the names-symbols difference is worthy of some consideration. The name run matrix was (with the exception of the first two weeks of Series I) located to the left of the symbol run matrix on the response cards, and thus was most likely to be filled in by subjects first. The difference in scoring obtained may have been due, for the most part, to this position factor. (Incline as well as decline effects have been reported in the literature.) Ideally, it would have been best if the position of name runs and symbol runs had been systematically alternated throughout the series. Any repetition of this experiment should include such alternation, so that the influence of a position factor, if present, may be eliminated.

The fluctuations in forward displacement scoring from series to series were sufficiently large to attract attention. It will be interesting to note further variation in displacement scores in future experiments. Conjecture as to the cause and meaning of these variations at this time is hardly profitable.

Conclusions

The results obtained in three series of experiments suggest that the use of proper names as targets in a GESP distance test will tend to cause mean run scores to fall below expectation. They also suggest (less positively) that such a tendency will be stronger where the proper names used involve close family relationships.

No conclusions can be drawn concerning the influence of the psychological nature of the targets.

Appendix

As described under "Procedure," the experiment consisted of setting up each night, as targets, two 5 x 5 squares (I will designate them as A and B). Entries into each of the 25 cells of A were selected from five possibilities by means of a table of random numbers. The cells of square B were similarly filled with selections from five other possibilities. A total of n (= 60) such targets were set up during the experiment (20 in each series). Of the 60 targets, there were five exceptions in which the composition of the squares was determined in advance (that is, there were exactly five of each

possibility in a square) and only the order was randomized. (We will refer to these five as closed targets.) From three to six subjects attempted to duplicate each target that was set up.

In the following discussion, we will make use of these notations:

A.D.—Attempted duplication (of a target-square)—i.e., one run.

Score of an A.D.—The number of cells in an A.D. which actually duplicate the corresponding cells of its target-square.

H_0—The hypothesis that there is no relationship between A.D.'s and their target-squares; that is, that only chance determines the scores of the A.D.'s.

We seek to determine whether the difference observed between rates of success in duplicating target-squares A and B (names and symbols) was a real one or merely due to random fluctuations.

In dealing with our data, let us first assume that attempted duplications of the same target-square are independent of one another.

Let X_{ij} = the score of the jth A.D. of the ith A-square. $\quad i=1,2,\ldots,n \quad j=1,2,\ldots,K_{A_i}$

Y_{ij} = the score of the jth A.D. of the ith B-square. $\quad i=1,2,\ldots,n \quad j=1,2,\ldots,K_{B_i}$

K_{A_i} = the number of subjects attempting to duplicate the ith A-square.

K_{B_i} = the number of subjects attempting to duplicate the ith B-square.

Then, under H_0, these will be independent observations on a binomially distributed (except where closed targets are involved) random variable with mean 5 and variance 4. When the target-square is closed, the distribution is conditional upon the relative frequencies of the five possibilities in the A.D. (1). However, the mean of these conditional distributions is always 5, and the variance is

$$\frac{1}{120}\left(625 - \sum_{k=1}^{5} m_k^2\right)$$

where m_k = the number of times that the kth possibility appears in the A.D.

which has a maximum of 4.17.

To answer the question which is of primary interest to this research, we are concerned with the statistic $(\overline{X} - \overline{Y})$ where:

$$\bar{X} = \frac{\sum_{i=1}^{m} \sum_{j=1}^{K_{A_i}} X_{ij}}{\sum_{i=1}^{m} K_{A_i}} \quad \text{and} \quad \bar{Y} = \frac{\sum_{i=1}^{m} \sum_{j=1}^{K_{B_i}} Y_{ij}}{\sum_{i=1}^{m} K_{B_i}}$$

Under H_0 and our assumption of independence, this statistic has expectation zero and variance equal to

$$\left(\frac{\sum_{i=1}^{m} \sum_{j=1}^{K_{A_i}} \operatorname{var} X_{ij}}{\left(\sum_{i=1}^{m} K_{A_i} \right)^2} + \frac{\sum_{i=1}^{m} \sum_{j=1}^{K_{B_i}} \operatorname{var} Y_{ij}}{\left(\sum_{i=1}^{m} K_{B_i} \right)^2} \right)$$

The exact distribution of $(\bar{X} - \bar{Y})$ is partially conditional upon the number of times each possibility appears in the A.D.'s of the closed target squares. Since the number of name runs, ΣK_{A_i}, and the number of symbol runs, ΣK_{B_i}, are fairly large, the distribution of $(\bar{X} - \bar{Y})$, after standardization, is approximate with a sufficient degree of accuracy for the purpose at hand, by the normal distribution with mean zero and unit variance. Thus, by standardizing our observed value of $(\bar{X} - \bar{Y})$, and using tables of the normal probability integral, we may test the hypothesis with which we are concerned, namely, that $E\bar{X} = E\bar{Y}$. If the test should result in a rejection of this hypothesis,[2] it is clear that we must also reject H_0.

Suppose we do not assume independence between attempted duplications of the same target-square. That is, it is conceivable that since names, personal to the subjects, were among the possi-

[2] We use as our critical region the two tails of the normal distribution containing together 1% of the probability.

bilities employed, subjects might tend to duplicate target-squares in similar patterns.

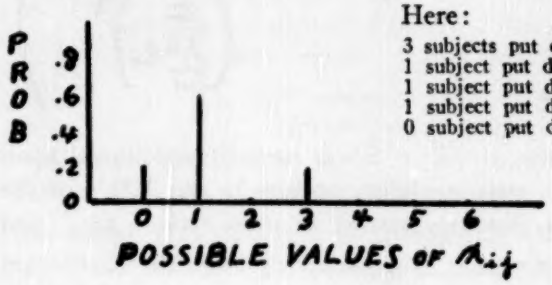
For those target-squares which are not closed, let

x_{ij} = the number of subjects who, in the jth cell of their A.D. of the ith A-square, put down the possibility which is actually chosen for the jth cell of the ith A-square. $0 \leq x_{ij} \leq K_{A_i}$

y_{ij} = the number of subjects who, in the jth cell of their A.D. of the ith B-square put down the possibility which is actually chosen for jth cell of the ith B-square. $0 \leq y_{ij} \leq K_{B_i}$

$$i = 1, 2, \ldots, n\text{-}5$$
$$j = 1, 2, \ldots, 25$$

Under H_0 these are independent observations on discretely distributed random variables whose distributions are conditional upon the relative frequency among the subjects of the five possibilities. For example:



Here:
3 subjects put down the 1st possibility
1 subject put down the 2nd possibility
1 subject put down the 3rd possibility
1 subject put down the 4th possibility
0 subject put down the 5th possibility

$$Ex_{ij} = \frac{6}{5}$$
$$\text{var } x_{ij} = \frac{24}{25}$$

In general

$$Ex_{ij} = \tfrac{1}{5} K_{A_i} \qquad Ey_{ij} = \tfrac{1}{5} K_{B_i}$$

$$\text{var } x_{ij} = \tfrac{1}{5} \sum_{k=1}^{5} m_{A_{ijk}}^2 - \left(\frac{K_{A_i}}{5}\right)^2 \qquad \text{var } y_{ij} = \tfrac{1}{5} \sum_{k=1}^{5} m_{B_{ijk}}^2 - \left(\frac{K_{B_i}}{5}\right)^2$$

where $m_{A_{ijk}}$ = the number of subjects who enter the kth possibility into the jth cell of the ith A-square.

and $m_{B_{ijk}}$ = the number of subjects who enter the kth possibility into the jth cell of the ith B-square.

We then know the distribution, mean, and variance of

$$X_i = \sum_{j=1}^{25} r_{ij} \qquad \text{and} \qquad Y_i = \sum_{j=1}^{25} r_{ij}$$

When the target-squares are closed, X_i and Y_i, still defined as above, are discretely distributed, as discussed in a paper by Greville (2), and

$$E X_i = 5 K_{Ai} \qquad E Y_i = 5 K_{Bi}$$

$$\operatorname{var} X_i = \frac{1}{120} \sum_{K=1}^{5} \left(25 \lambda_{AiK} - m_{AiK}^2 \right) \qquad \operatorname{var} Y_i = \frac{1}{120} \sum_{K=1}^{5} \left(25 \lambda_{BiK} - m_{BiK}^2 \right)$$

$$\lambda_{AiK} = \sum_{j=1}^{25} m_{Aijk}^2 \qquad \lambda_{BiK} = \sum_{j=1}^{25} m_{Bijk}^2$$

$$m_{AiK} = \sum_{j=1}^{25} m_{Aijk} \qquad m_{BiK} = \sum_{j=1}^{25} m_{Bijk}$$

For all squares, closed and not closed, the statistic with which we are concerned is the same quantity as before, that is $(\overline{X} - \overline{Y})$, where we now consider

$$\overline{X} = \frac{\sum_{i=1}^{m} X_i}{\sum_{i=1}^{m} K_{Ai}} \qquad \text{and} \qquad \overline{Y} = \frac{\sum_{i=1}^{m} Y_i}{\sum_{i=1}^{m} K_{Bi}}$$

and where now the distribution and variance of the statistic, under H_0, are different than before, although its expectation is still zero. The exact distribution of $(\overline{X} - \overline{Y})$ is conditional upon the relative

frequency of the possibilities in the A.D.'s. Again, because of the fact that the number of cases with which we are dealing is fairly large, the distribution of standardized $(\overline{X} - \overline{Y})$ may be approximated by the normal zero-one distribution. Using our observed value of $(\overline{X} - \overline{Y})$ we may test the hypothesis that $E\overline{X} = E\overline{Y}$, and again, rejection of this hypothesis implies rejection of H_0.

In Table 4, we compare the computed variances of $(\overline{X} - \overline{Y})$ with and without the assumption of independence between A.D.'s of the same target-square. The comparison indicates that the assumption is a reasonable one, and it is therefore made upon further breakdown of the data into relative and non-relative categories (see "Results").

Table 4

Series	Number of Name Runs ΣK_{A_i}	Number of Symbol Runs ΣK_{B_i}	VAR $(\overline{X}-\overline{Y})$	
			Independence between A.D.'s of the same Target-Square	
			Assumed	Not Assumed
I*	92	94	0.086458	0.082089
II	112	112	0.071429	0.071126
III	95	95	0.084210	0.086821
II and III	207	207	0.038647	0.039108
I, II, and III	299	301	0.026708	0.026506

*Series I contains all five closed targets.

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GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

BT ("Broken Technique"): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards.

CLOSED DECK: An ESP deck composed of five each of the five symbols.

OPEN DECK: An ESP deck made up of the ESP symbols selected in random order.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

BACKWARD DISPLACEMENT: ESP responses to targets preceding the assigned targets (the ones for which they were intended). Displacement to the targets

one, two, three, etc. places preceding the assigned target are designated as (—1), (—2), (—3), etc.

FORWARD DISPLACEMENT: ESP responses to targets coming later than the assigned targets. Displacement to the targets one, two, three, etc. places after the assigned target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the deck before any are removed or checked.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibility of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single trials, p the probability of success per trial, and q the probability of failure. (For ESP cards, SD $= 2\sqrt{}$no. of runs.) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ_s is the SD of a single run and R_1 and R_2 are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in this JOURNAL is a probability value of .02 or less, or a deviation in either direction such that the CR is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.

be a fact, it would be a wedding of the ideas of Darwin and Mendel on the one hand and of Lamarck and Samuel Butler on the other!

If I appear to be ending in fantasy or in the spirit of Harlequinade, I do so only to emphasize my conviction that we fool ourselves if we imagine that our present ideas about life and evolution are more than a tiny fraction of the truth yet to be discovered in the almost endless years ahead. We are but at the threshold of our understanding of living things. It is indeed urgent that we should press on. I have tried to suggest that if resources similar to those put into economic ecology were available for evolutionary studies in the field, we now have methods capable of giving us quantitative and reliable answers to our questions. I am sure that zoology outside the laboratory will play a part in increasing our knowledge no less important than that played by zoology inside the laboratory, but I am also sure that sound progress will only be made by maintaining a balance and a close liaison between the two.

—A. C. Hardy, "Zoology Outside the Laboratory,"
The Advancement of Science, Vol. 6, 1949.

TELEPATHY AND EVOLUTION

There is another matter which I feel it only right to mention if one is not to be intellectually dishonest. There has appeared over the horizon something which many of us do not like to look at. If it is pointed out to us we say: "No, it can't be there, our doctrines say it is impossible." I refer to telepathy—the communication of one mind with another by means other than by the ordinary senses. I believe that no one, who examines the evidence with an unbiased mind, can reject it; particularly the evidence of the vast number of experiments of the past twenty years and of that group of them which has passed successfully the statistical tests regarded as decisive when applied in normal scientific technique.

It is perhaps unorthodox for a zoologist to introduce such a topic; but I do so for a reason. If telepathy has been established, as I believe it has, then such a revolutionary discovery should make us keep our minds open to the possibility that there may be so much more in living things and their evolution than our science has hitherto led us to expect. Such an idea as I am about to suggest is no doubt highly improbable and would perhaps be better kept locked in a bottom drawer; I mention it however merely as a reminder that perhaps our ideas on evolution may be altered if something akin to telepathy—unconscious no doubt—was found to be a factor in molding the patterns of behavior among members of a species. If there was such a non-conscious group behavior plan, distributed between, and linking, the individuals of the race, we might find ourselves coming back to something like those ideas of subconscious racial memory of Samuel Butler, but on a group rather than an individual basis. It would remove many of the fatal difficulties of his hypothesis. Samuel Butler's ideas were of course the logical development of Lamarck's, but thought of independently. If there was such a group habit and behavior pattern it might operate through organic selection to modify the course of evolution: working through selection acting on the gene-complex. If this flight of fancy ever proved to

(Continued on inside back cover)